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ACADEMIC SCIENTIFIC MEN AND CONSULTANT WORK

IN the Nuffield College Statement on "Problems of Scientific and Industrial Research", as well as in other reports in which the relations between industry and university men of science have been considered, the question of the extent to which university scientific workers should undertake advisory or research work on behalf of private firms and receive payment for such work has been discussed in some detail. The important part which the academic man of science has played in the team-work which has been responsible for so many of the striking developments during war-time—of which we need instance no more than radar, the utilization of atomic energy and penicillin—make it almost certain that the nation will be reluctant to forgo in the post-war years a partnership which proved so fruitful and valuable. Moreover, apart from the value of such co-operation in stimulating creative work, the shortage of scientific man-power and the necessity of using our resources to the best advantage will provide a further impetus to closer relations between industry and the universities; and the more especially since, to provide the increasing number of research workers and other scientific and technical men which industry will need, it will probably be necessary for a time to give the staffing of university departments for teaching and research a priority over industrial demands.

Under such conditions, it will be imperative to maintain an unusually careful watch to ensure that there is no neglect of fundamental research at the universities, and that no practices are allowed to develop which would involve any restrictive conditions or diversion from that primary function. Even if every precaution is taken in that respect, problems are bound to arise in peace-time which did not arise, or not to the same extent, in the prosecution of co-operative research in connexion with the war effort. Questions of publication or the exchange of information were so dominated by the overriding necessity of avoiding disclosure to the enemy that the questions of patenting or priority which would normally arise in development in peace-time were of secondary or negligible interest.

There can be little doubt that industry as a whole appreciates the vital importance of the fullest and widest publication of the results of fundamental research to a much greater extent than was the case a generation ago, and there is no evidence that British industry is any more reluctant to publish the results of scientific investigations carried out in its own research laboratories than is American industry. While it is therefore improbable that there will be any disposition on the part of industry to withhold permission to publish the results of investigations carried out in university laboratories at its instigation or charges, there may still be some danger of university departments falling, even unconsciously, too much under the influence of particular firms or industries. Accordingly, the formulation of a general

code of conduct recognized as applying to university scientific workers undertaking outside industrial research for private firms is even more imperative than when the Nuffield Statement was first issued, and especially in view of the general agreement which is now evident as to the desirability of much easier and greater interchange of research workers between industry, the universities and Government departments.

It has been suggested that the professional institutions should take the initiative in formulating the principles of such a code of practice. It is therefore welcome news that the Joint Council of Professional Scientists has prepared a report* on the subject which was approved last November both by the Council of the Royal Institute of Chemistry and the Board of the Institute of Physics. This report sets forth the reasons why, and the occasions when, in the view of the Joint Council, consulting and similar work should be undertaken by academic men of science, and details five principles by which acceptance of such work should be guided. Of the reasons given, the first will command general agreement: it is clearly in the national interest that industry should be able to obtain the best and most up-to-date scientific advice, and no scientific worker able and willing to provide such advice or assistance should be unreasonably restrained from doing so. It is also true that men of science employed by a firm, or by a research association of which it is a member, are unable to meet all demands. Even with a greater expansion of industrial research, the resources of the smaller firms may still be wholly inadequate.

While such circumstances may become less frequent, some external help may legitimately be needed and much of it would naturally come from members of the staffs of universities and technical colleges. It may, of course, also come from independent consultants, and recent inquiries made by the Royal Institute of Chemistry show that a very wide range of advice or assistance can now be obtained from independent consultants in chemistry and chemical engineering. Such consultants may have specialized knowledge and experience in particular fields or in relation to particular branches of industry, and have at their command special apparatus and equipment for particular kinds of experimental work; but in other branches of science, except those special sections of engineering which may be regarded as applied physics, the position is different.

There may be sensed a general tenderness for the independent practitioner which leads to the consultant receiving rather more prominence in this matter than is justified, the more particularly as it might well be argued that as industry pays more attention to research there will be a diminishing call for his services. None the less, it is undesirable that members of the staffs of universities and technical colleges should compete directly with independent practitioners, except in fields where the latter are clearly unable to meet the needs of industry. Any such consulting work undertaken should, moreover, not be such as to interfere with the proper discharge of the primary duties of the university man of science of teaching and of advancing his science by the prosecution of research. Since, however, the services required by an industrial firm may range from routine testing to long-range investigations of an essentially academic nature, and vary in purpose from settling a specific scientific question to developing a new process to the stage of full industrial operation, such consulting work may often accord with the discharge of these primary duties at least as well as it is likely to do with the services which the independent consultant can render. Apart from this, it is recognized that the knowledge of industrial and other modern applications of science which the academic man of science can gain by engaging in some outside work may be of considerable benefit to his teaching and research in fundamental science. Many academic men of science, by virtue of their knowledge and experience in specialized fields, have important contributions to make to industrial development which could not be provided from other sources. Universities and technical colleges also often possess special apparatus and equipment which is not otherwise available, and it is in the public interest that its use should be made available, particularly to local industry.

The first principle which, it is suggested, should guide the acceptance of outside consulting work by scientific workers in academic institutions is that the work should be investigatory in character; routine testing should be undertaken only in very exceptional circumstances. Secondly, when research work is carried out in an academic institution in connexion with the consulting work, the scientific results should normally be published, subject to an agreed delay to allow the firm paying for the work to have priority in the application of the results, and subject to collaboration with the firm being duly acknowledged. Thirdly, no agreement should be made with a firm which would restrict the services of a man of science in an academic institution to any one firm, except in respect of the specific problems considered. Fourthly, when a fee is paid, the remuneration of those who have shared in the work should be decided on the particular merits and circumstances, having regard to any conditions laid down by the governing body of the academic institution concerned; and fifthly, if there is a possibility of patents arising from the work thus carried out, the arrangements to be made with the sponsors of the work should be considered in the light of the normal practice of that body.

The application of some of these principles might have to be modified with reference to research or development work undertaken for Government departments. Moreover, the statement suggests that it is reasonable that some control over such paid outside work as regards its suitability, non-interference with the performance of teaching and research duties and suitable payment for use of the institution's equipment should be exercised by the employing institution, normally acting through the chief academic official. This is in accordance with criticism

* Copies of the report are obtainable from the Joint Council of Professional Scientists, c/o The Institute of Physics, 19 Albemarle Street, London, W.1.

advanced by Bruce Truscot in his two books on "Redbrick University". While such control need not, in theory, be irksome or derogatory to members of the staff, as their approach would normally be made through the head of the department to the chief academic official, whose opinion regarding the proposed acceptance of outside work should be easily ascertainable, nevertheless in practice, if Bruce Truscot is right, the danger of friction cannot be discounted. The decision should be reported to the body representing the employing authority, to which, if necessary, the member of the staff would also have direct approach; and in any event modification of the administrative structure of the universities, where necessary, in the direction suggested by Bruce Truscot for other reasons should go far to remove the difficulty here.

Copies of the report have already been sent to the vice-chancellors and principals of universities and colleges, and it equally deserves the attention of industrialists and other professional bodies besides those represented on the Joint Council of Professional Scientists, as an encouraging attempt to frame a clear code of professional conduct in a field which is likely to become increasingly important in the years immediately ahead.

ESSAYS ON GROWTH AND FORM

Essays on Growth and Form presented to D'Arcy Wentworth Thompson

Edited by W. E. Le Gros Clark and P. B. Medawar. Pp. viii+408+13 plates. (Oxford: Clarendon Press; London: Oxford University Press, 1945.) 21*s.* net.

THE production of a *Festschrift* is not a usual practice in Britain; but there are some men whose influence has been so decisive in stimulating the widespread exploration of a scientific problem which they were unequivocally responsible for pointing out, that a tribute to their insight comes spontaneously to the minds of their followers when the occasion arises. Few British biologists who are concerned with the general problems of anatomy would hesitate to include D'Arcy Thompson in this category, or to feel a wish, on some suitable occasion, to make an acknowledgment of their debt to him. The occasion which has been chosen for the presentation of this volume of studies is the completion of his sixtieth year as professor, first in University College, Dundee, which later became incorporated in the University of St. Andrews.

During this long incumbency of his chair, D'Arcy Thompson has written with scholarship and grace on many subjects; he has shown himself an erudite classicist as well as a mathematician, field naturalist, oceanographer, pedagogue and archæologist. But it is in the field of analytical anatomy that his writings have had their most important influence on the work of his younger contemporaries. The publication, in 1917, of the first edition of "On Growth and Form" presented biologists with a demonstration both of the nature of the analytical approach to these problems and of the wide range of material to which such methods could be applied. Before turning to the volume of essays under review, which shows something of what biologists have achieved in following up D'Arcy Thompson's suggestions, it will be well to examine more closely what were the main directions of inquiry which he indicated. The most important were, perhaps, three in number. "On Growth and Form" brought together into a single extended discussion the many diverse attempts which had been made to give a mathematical account of the phenomenon of growth, or simple increase size or weight. Secondly, it showed that many examples of animal form are geometrically similar to configurations which are taken up by non-living materials of various kinds under the action of known physical forces. Thirdly, it suggested the value of using the technique of transformation of co-ordinate systems to summarize the comparison between the shapes of related organisms. The common characteristic of all these lines of thought, and the characteristic which gave them their power to inspire other workers, was an insistence on the necessity for mathematical analysis. In the essays now presented to D'Arcy Thompson, nearly thirty years later, the mathematical equipment is used frequently and unself-consciously by biologists who have, in the interval, come to accept its value as a matter of course. But, in nearly all the essays, one notices the authors being driven, by the nature of their material, towards the adoption of two points of view which were only partially adumbrated in the Thompsonian directive; these are, first, a concern that the analysis shall be in terms of entities which are causally significant, and secondly, a conception of animal shape as essentially in process of development.

There are, admittedly, certain essays which do not show these two new characteristics. Lotka contributes a brilliant account of "Population Analysis as a Chapter in the Mathematical Theory of Evolution", a subject which D'Arcy Thompson[1] judged to "lie just outside the scope" of his book, but which the reader will welcome none the less. Astbury again, in his general, and as he says somewhat informal, account of "The Forms of Biological Molecules" is dealing with a subject which did not fall within D'Arcy Thompson's purview—indeed could not, since what Astbury is recounting is the advance made during the last twenty years in our understanding of the ultimate entities from which biological forms are constructed.

It is in the series of essays on growth that the inspiration of Thompson is most direct, and at the same time the necessity to develop beyond his point of view most pressing. Reeve and Huxley on "Some Problems in the Study of Allometric Growth", Medawar on "Size, Shape and Age", and Richards and Kavanagh on "The Analysis of Growing Form", are all concerned to carry forward the mathematical expression of growth, and to synthesize this with the other strand of D'Arcy Thompson's thought, the geometrical consideration of changes in shape. Much progress has been made in both endeavours, and a considerable body of knowledge has grown up around the original nucleus. But all the authors conclude that the situation is still as it was when Gray made his penetrating analysis in 1929; no analytical expression for the curve of growth can be found to which one can attach a causal, as opposed to a purely descriptive, significance. In all three essays, the Thompsonian analysis of changes of shape is applied not only to the comparison of related forms, but also to the development of a single individual; but again, even in the detailed study by Richards

and Kavanagh of the simple system of a growing tobacco leaf, the regularities which appear are of purely descriptive value. To the reviewer, at least, it emerges from every example discussed that the next step in these studies waits on the formulation of a physiological theory of growth. In an organ which is changing its shape, we are confronted with an orderly arrangement of different rates of synthesis. We have at present almost no idea how these rates are controlled. It may be that comparatively simple general laws would emerge from a study of longer phases of development; most of the investigations deal only with an embryologically late stage in which the growth process is running down, and it is conceivable that, if earlier stages were studied, it would be found that the differences in rate of various parts are dependent on the time at which differentiation occurred or some other simple parameter. But it is perhaps more likely that no satisfactory formulation will be found until we understand more fully the mutual interactions between the processes by which the cytoplasm is synthesized and the genetic factors the doubling of which initiates cell-division.

The physiological approach has made more headway in relation to another of D'Arcy Thompson's legacies—the study of organic form as a resultant of physical forces. Whereas thirty years ago it was a revelation to see, in the pages of "On Growth and Form", the geometrical analogies between groups of cells and soap-bubbles, or hydroid polyps and water splashes, nowadays the appropriate physical theories are accepted tools in investigations which are firmly founded on thorough experimental study. Essays on such lines are contributed by Le Gros Clark ("Deformation Patterns in the Cerebral Cortex"), Wigglesworth ("Growth and Form in an Insect"), Young ("The History of the Shape of a Nerve-Fibre"), Berrill ("Size and Organisation in the Development of Ascidians"), Willmer ("Growth and Form in Tissue Cultures") and Danielli ("Some Reflections on the Forms of Simpler Cells").

The wide range of topics touched on in this volume of essays is appropriate in a tribute to so catholic a biologist as D'Arcy Thompson. But, diverse though their fields of study may be, all the authors show, not merely by expressions of gratitude but also in the actual methods of thought by which they are guided, the influence of the master to whom the book is dedicated; a fact which is not only a justification of the decision to prepare a *Festschrift*, but also a cause for congratulation to the editors.

C. H. WADDINGTON.

"On Growth and Form", 2nd ed., p. 159.

LORD SAMUEL'S MEMOIRS

Memoirs

By the Rt. Hon. Viscount Samuel. Pp. viii+304. (London: Cresset Press, Ltd., 1945.) 15*s*. net.

AMONG that small band of statesmen, including Lord Balfour and Lord Haldane, through whose efforts and advocacy in the last thirty years science and scientific research have come to occupy in Great Britain a more fitting place in the life of the nation, Lord Samuel holds an assured place. In debates on scientific research, the organization of the war-effort, the planning of national resources, whether of land or of man-power or brain-power, Lord Samuel has consistently put the scientific point of view, lucidly and forcefully, but never overstated. It would be difficult to determine how much scientific workers owe to his reasoned presentation of their case in such matters as the recent proposals for a Scientific Civil Service or the organization of science for war, but of all this there is little hint in these memoirs. There is a passing reference to his position as president of the Parliamentary and Scientific Committee, and there are accounts of the origins of his two books, "Belief and Action" and "An Unknown Land", which reveal his deep interest in both science and philosophy.

While there is little here that touches directly on scientific matters, the scientific outlook which pervades the book is unmistakable. Through the great events of which he writes, and on which he sometimes throws fresh light, Lord Samuel moves with ease and judgment. Here are displayed the qualities of mind demanded of anyone who would fill the post of Minister of Science for which Lord Samuel has pleaded so eloquently. He writes, moreover, with charm and dignity and the quiet intimacy of a fireside talk. Pen pictures as delightful as his account of Barrie's luncheon party to Lord Grey and his own tribute to Grey are illuminated as by flashes of firelight through his flair for the vivid and telling phrase. He can express Lloyd George's personality in his prime in one vivid sentence: "He seemed to be marching through life, head high, to the strains of 'The Men of Harlech' ", but Lord Samuel is never betrayed by that gift into the superficial. In a brilliant chapter on the Cabinet of 1914, he gives us penetrating sketches of Lloyd George and of Churchill —to whom alone of the statesmen of our time he attributes the touch of genius—and others of his colleagues which make one wish that this Messel medallist might give us a like gallery of sketches of men of science.

In this volume there are indeed already sketches of Einstein and of Weizmann, and the man of science who stays to savour the quality and dignity of Lord Samuel's limpid prose can scarcely fail to appreciate also the whole philosophy and spirit of the book. Lord Samuel possesses the judicial temperament, and neither his loyalty to his colleagues nor his passion for impartiality blunts the edge of a discerning mind. He possesses in large measure that equanimity which he notes as Asquith's fundamental quality; and the sincerity and unflinching loyalty to the spirit of truth which illumine the pages of this book from first to last, with the quiet dignity of its style, should give it an assured place in literature.

R. BRIGHTMAN.

PULMONARY ŒDEMA AND INFLAMMATION

Pulmonary Edema and Inflammation

An Analysis of Processes Involved in the Formation and Removal of Pulmonary Transudates and Exudates. (Harvard University Monographs in Medicine and Public Health, No. 7.) By Prof. Cecil K. Drinker. Pp. ix+106+17 plates. (Cambridge, Mass.: Harvard University Press; London: Oxford University Press, 1945.) 2.50 dollars.

PROF. DRINKER has undertaken in this short monograph of five lectures to explain some of the physiological and pathological processes involved in

the production, perpetuation, and treatment of pulmonary œdema and inflammation.

The first lecture describes the capillary circulation in the lungs and its powers of reaction to imposed stresses. It describes also the lymphatic system which is concerned in some degree with the removal of exudates and transudates, and the alveolar pores and their role in collateral ventilation. In the second lecture it is demonstrated that anoxia causes excessive capillary leakage in the lungs as elsewhere; that increased pressure in the pulmonary capillaries is not, alone, a cause of recognizable œdema; and that localized patches of œdema can and do easily occur in the lungs without noticeably affecting breathing. An interesting new compound, allied to thiourea, has been found to be a specific for causing pulmonary œdema and pleural effusion in dogs. By the use of this drug it has been demonstrated that the lymphatics have a limited power in removing exudates, and beyond this point the only outflow is by the air passages—a route which implies progressive exclusion of air and eventual death.

The third and fourth lectures, from the above basis, provide a discussion on the effects of breathing movements on transudation and on the therapeutic approach to pulmonary œdema. Oxygen should be given early in lung diseases characterized by anoxia and not used only as a last resort when the patient is blue. Pure oxygen should be administered by a non-leaking mask in half-hour spells with equal rests between. Morphia and other sedatives, although soothing to the patient, do harm by reducing the range of breathing and increasing anoxia.

A final and most instructive lecture deals with the various methods of artificial respiration, manual and mechanical, applicable in different circumstances. In emergencies the author gives pride of place to the mouth to mouth method, with Shäfer's method next and Silvester's last. Mechanical devices such as the pulmotor, which provide alternate pressure and suction, are liable to defeat their own ends if used for long periods, as suction at the expiratory phase gradually reduces the aerating surface of the lungs. The Drinker-Shaw apparatus which allows passive expiration is the best for cases which must remain long under treatment. Of theoretical interest is Thunberg's barospirator, in which alteration of air pressure is used instead of alteration of volume, so that respiration may take place without the movements of breathing.

Like all the Harvard monographs in medicine this is interesting and concise, and the book is well produced and illustrated. JAMES MARSHALL.

MAN THE MICROCOSM

Man and his Meaning

By Dr. J. Parton Milum. Pp. 125. (London: Skeffington and Son, Ltd., 1945.) 15s. net.

THE entity known as mind, so far as appearances go, would seem to exist only in certain highly developed organisms—man, and possibly to some degree in the higher mammals. The actual nature of mind is much of a mystery, and likely to remain so. The generally accepted theory is that it has been developed in the creature to enable it to cope successfully with its environment. On this view it would not be an instrument capable of solving the mysteries of the universe. Nor would a mind-endowed species be entitled to regard itself as of peculiar importance in the scheme of things.

Yet to confine mind to its familiar organic embodiments may not after all be justified, as was surmised by Plato, who put into the mouth of Socrates words quoted by Dr. Parton Milum, which still deserve attention:

"Do you suppose that you have a little wisdom yourself, and yet that there is no wisdom to be found elsewhere? And that, too, when you know that you have in your body only a small fragment of the mighty earth and a little portion of the great waters; and of the other elements you received, I suppose, a little bit of each towards the framing of your body? Mind alone forsooth, which is nowhere to be found, you seem by some lucky chance or other to have snatched up from nowhere!"

Dr. Parton Milum argues in his book that "the insight which man has into nature is possible only because his essential self is akin to the fundamental Reality in nature". It is not man who has projected his mind upon the universe, but rather the order implicit in the universe has moulded and stamped itself upon man's mind. In fact, "man's mind is the reflex of the order outside himself in the universe".

The issue as to whether mind actually is limited to its embodiment in man is more than a speculative question, because when man can believe that a certain congruity exists between his own nature and the universe, he has confidence in his own destiny and value in the scheme of things. Indeed, recent events have established the truth of Dr. Parton's words when he writes:

"Until the dignity and worth of human nature are perceived to be grounded in Reality, all the amazing gifts of science tend to neutralize themselves and may become actually destructive of human good."

In Dr. Parton Milum's view, the ancient idea that man is a microcosm, or little world in himself, developing in response to the macrocosm or universe, and having its counterpart in his own being, is a far more adequate conception than the prevailing attempt to envisage man as an evolving animal; and he reminds us of the answer offered by the Christian religion to the enigma of man's nature, namely, that "Man is man by virtue of something which he only partially possesses. The truly human which he does not wholly possess is not something derived from the animal world, but is of a cosmic and spiritual nature". Moreover, against the denial that the universe sets any value upon the human individual, since value can only be attached to any object by a person, Christianity affirms that the basis of the universe itself is personal.

Dr. Parton Milum holds that it is impossible to think about the universe apart from a subjective reference, which subjective reference is grounded in reality at least as much as the 'external' world. That "the things which are seen" bear witness to an invisible order is not only an intuition of primitive man, but is also the assumption upon which the scientific view of the universe has been built up.

This book is a useful antidote to the superficial view held by many that there is nothing in the universe but matter, that mind is a phenomenon of matter and dependent upon it—a theory which, as F. H. Bradley pointed out, is a mutilation of the human spirit; it is also a short road to barbarism.

J. C. HARDWICK.

HISTORICAL DEVELOPMENT OF THE BETATRON

By Prof. DONALD W. KERST
University of Illinois

THE magnetic induction equation of electrodynamics, curl $E = -\frac{\dot{B}}{c}$, has suggested to many that a free charge could be caused to acquire great energy if it were allowed to circulate about a varying magnetic field. It suggested to W. F. G. Swann[1] that fluctuations in the magnetic field in interstellar space might be responsible for the acceleration of free electrons to the energy of cosmic rays. It likewise suggested an attempt made in 1904 by G. F. Hull to detect the mechanical force of this electric field on an electrostatically charged cylinder surrounding a varying magnetic induction.

Of increasing practical interest are the attempts to use a changing magnetic induction for the controlled acceleration of electrons for medicine, industrial work, and physics. A large number of careful experimenters attempted to generate high-energy electrons by this method. Since difficulties were encountered, and since results were in general discouraging, much that has been tried is unpublished. There are, however, traces of the progress of the idea in occasional publications. It is the plan of this article to summarize those historical steps of which I am aware and which have led to the development of the present-day betatron. The literature on the subject is scarce, and some published investigations may be unknown to me. I am sure that because of this scarcity some of the investigators have independently made progress in this field without a full knowledge of the previous developments.

The idea of accelerating electrons by magnetic induction seems to have occurred first to J. Slepian in 1922, when he suggested it as a method of accelerating the electrons emitted from a filament in a high vacuum[2]. Slepian pointed out that if an electron were attracted by the electrostatic pull of a target, it would tend to spiral about the lines of force of a magnetic field; and, if this magnetic field were increasing, the electron would be accelerated. This he imagined as a method of increasing the energy of electrons in an X-ray tube without the use of high potentials. He was apparently aware that with this simple spiralling orbit very little energy could be gained, for he went on to describe a second system very much like a transformer, with the central leg of the transformer linking the orbit and with permanent magnets placed within the 'windows' of the transformer to bend the electron path around the central leg (Fig. 1). If an electron orbit could be created in his machine, then as long as the flux linking it rose, there would be an induced electromotive force about the orbit which would accelerate the electrons. When the peak flux was reached, the electrons would no longer be accelerated and would somehow be utilized. Slepian did not develop the theory of his ideas to a great extent at the time of his suggestion.

The proposal of Slepian mentioned first was to start the electrons in the centre of a uniform and constant magnetic field by the electrostatic pull of a distant target, and to superimpose on this steady magnetic field a time-varying magnetic field. Unless a very small energy is desired, the time-varying magnetic field must be made large, and then the electrons spiral back inward to the centre, making smaller loops and utilizing the flux poorly. His second system with the transformer core linking the orbit is a method of gaining very high flux linkage; but, as he described it, the permanent magnets which bend the electron path about the core were arranged to give a strong field at large radii and a weak field at small radii, so that as the electrons gained energy and spiralled outwardly, they would find themselves restrained from too great outward motion by the increasingly strong bending field from the permanent magnet. This shape of magnetic field is exactly the opposite of what is necessary for stability of the electron beam in the direction of the axis of the magnetic field. As applied in the earliest cyclotron, the method of achieving axial stability of a beam of charged particles was to have the magnetic field decrease slightly as the radius increased. This bulging field provides forces which always direct the straying particle back toward the plane of symmetry of the magnet.

Apparently Slepian did not pursue the idea, but his suggestion was nevertheless extremely important, and it was destined to be tried vigorously by many people and eventually to work on an unquestionably practical scale.

Later in the same decade, Breit and Tuve, at the Bureau of Terrestrial Magnetism in Washington, D.C., had been intensively examining various methods of producing high-energy particles for the purpose of bombarding atomic nuclei, and among the several methods which they tried was the method of magnetic induction[3]. The apparatus employed is very simple and seems to have great possibilities for producing intermittent bursts of X-rays. Their method was entirely different from Slepian's proposal. They constructed two large air solenoids and placed them end to end with the acceleration vessel between them. The plan was to shoot electrons from a gun into the region of an extremely rapidly increasing magnetic field caused by discharging a high-voltage condenser through the solenoids. The electrons were then to spiral inwardly while the field rose, and to

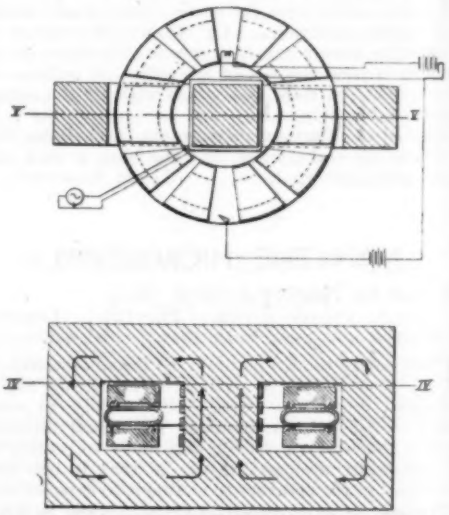

Fig. 1. Slepian's transformer-like induction accelerator. Permanent magnets are used to bend the orbit around the central leg of the transformer.

strike a target near the centre at a small radius from the axis of the solenoids. These investigators had examined theoretically the motion of the electrons, and they learned that in a uniform magnetic field which increases in time the electron chooses a path which links a constant amount of flux. Thus the initial momentum with which the electrons are shot into the acceleration chamber is multiplied by the ratio of the initial radius of the electron orbit to the final radius of the electron orbit; but they also realized the need for axial focusing; so the gap between the solenoids was arranged to produce a magnetic field which decreased slightly with increasing radius and therefore provided the necessary bulging magnetic field which would force the electrons back to the plane of their orbit. This is the important axial stability requirement used later in cyclotrons and present-day betatrons.

There was every reason to expect this arrangement to operate successfully for the production of bursts of X-rays, and the experimenters felt that the apparatus was able to produce X-rays, although their method of detection was difficult to manage in the presence of the sparking accompanying the discharge of the high-voltage condensers. Although some of the early investigators did not have correct methods of starting electrons, Breit and Tuve's arrangement was sound. For injection there are two necessary requirements. First, it is important to shoot the electrons into the acceleration vessel at a high enough energy so that scattering on the residual gas will not destroy the beam. This is not so important when the electrons quickly gain energy in a very rapidly changing magnetic field; but it is important in the cases of slowly rising magnetic fields. Secondly, injection must be performed so that the electrons do not immediately fly out of the magnetic field. Breit and Tuve could introduce their electrons from outside the magnetic field, because on the first revolution within the field they gained enough energy to spiral inward and to be captured by the magnetic field. In the case of the accelerator having a slowly rising magnetic field, the field is practically constant for the first few revolutions of the electron, and since the trajectory in a constant magnetic field is symmetrical about the point where $\dot{r} = 0$, the electron will fly out of the magnetic field if it started from the outside. The proper method of starting electrons in a slowly varying magnetic field was not discovered until the complete theory of the betatron was worked out and tested in 1939 and 1940.

The Bureau of Terrestrial Magnetism, where Breit and Tuve did their experiments, was soon to have a great deal of success with the Van de Graaff electrostatic high-voltage generator which they used extensively for nuclear investigations, and the efforts with their method of induction acceleration did not continue.

The next step in the development of the theory of electron acceleration was taken by R. Wideröe[4] in Germany, who carried out some very valuable experiments on the subject. His important theoretical contribution was to show how to arrange Slepian's iron core system with a slowly varying magnetic field to reach high flux-linkages and high energies. Wideröe's method is to link the orbits with the leg of an iron core transformer which contains an air gap in the plane of the orbit. The electrons travel in a magnetic field generated by the same coil which sends flux through the transformer leg, and this field thus increases in strength along with the magnetic

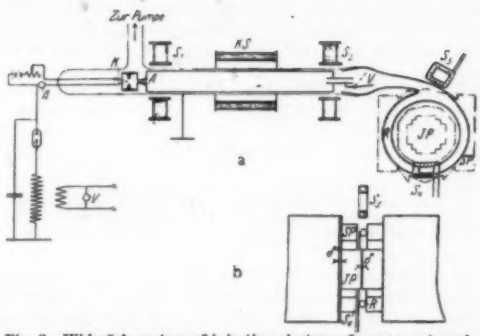

Fig. 2. Wideröe's system of injecting electrons from an external cathode-ray gun. The coil, S_3, is supposed to assist in bending the electrons into the circular vacuum tube, and the coil, S_4, is to assist in guiding the electrons farther around the tube, *R*. The central flux comes through the iron labelled *JP*. The magnetic field which bends the electrons into the orbit comes through the iron *SP*. Both *JP* and *SP* take their flux from the same magnetic yoke.

flux linking the orbit (Fig. 2*b*). Without this central flux from the transformer leg the orbits would spiral inwardly as in the Breit-Tuve machine. With exactly the right amount of flux led through the orbit by the transformer leg, the orbits could be caused to stay at a fixed large radius and to acquire very high energy as the field increased. This additional central flux linkage supplies energy to the electrons, which is not supplied by the approximately uniform field used by Breit and Tuve. The additional momentum gained in Wideröe's device requires that the electron shall travel at a large fixed radius.

Wideröe proved theoretically that, if the orbit has a fixed radius, the total flux, Φ, within the orbit must always be $2\pi r^2 H$, where H is the field at the orbit. The air gap introduced by Wideröe in the central leg of the transformer makes it easy to keep Φ and H increasing together with time. This flux condition of Wideröe's is a very important result which is put to use in the betatrons of to-day.

Wideröe did not heed the necessity for axial and radial focusing forces; but the type of machine which he built and tested cannot be much altered in iron-core betatrons, since one of the foremost requirements is the fulfilment of Wideröe's flux condition. It is very likely that somewhere in Wideröe's field both axial and radial focusing existed.

Wideröe attempted to inject the electrons by a cathode ray gun outside the magnetic field (Fig. 2*a*). However, the large transformer core which he used for the magnet structure was energized by closing a switch in a direct current supply, and the time constant for creating the magnetic field was two-tenths of a second. Thus electrons shot in from the outside would have very little chance of being captured by such a slowly changing field.

To extract the electron beam once it was accelerated to high energy, Wideröe placed coils near the acceleration tube for the purpose of deflecting the orbit out of the apparatus (S_4 in Fig. 2). While this is probably not a sound way of extracting the beam from the betatron, such a coil does have the property of forming in the magnetic field a bump which would scatter the electrons to and fro so that they could reach an X-ray target when they are widely enough scattered. Coils near the electron orbit energized at the proper time are used in modern betatrons for sending the electrons against the target.

Wideröe states that the walls of the glass vacuum

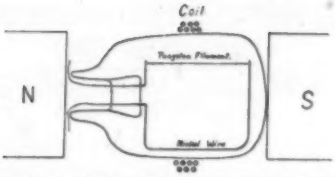

Fig. 3. A sketch of Walton's first experimental tube. The coil provided the time-varying magnetic field, and the magnet poles provided a constant magnetic field. The electrons were to start from rest at the tungsten filament and to strike the nickel wire and the walls eventually.

tube which he used accumulated charge and quickly approached the cathode potential. Thus the electrons which he injected were repelled, and he could get the beam to revolve only one and a half times around the tube.

In this very helpful account of acceleration experiments, Widerõe makes several suggestions as a result of his experiences. He feels that stabilization of electrons in the equilibrium orbit is the next important development, and he suggests electrodes around the circumference of the tube to produce electrostatic focusing, and also a magnetic concentration coil wound around the annular vacuum tube to pinch down the electron beam.

At about the same time that Widerõe was making his experiments, E. T. S. Walton[5] in Britain made what is perhaps the most valuable step forward until that time in the understanding of the achievement of stability. At the suggestion of Lord Rutherford, Walton made a careful theoretical and experimental examination of induction acceleration. The main theoretical problems with which Walton concerned himself were the discovery of the condition for an equilibrium orbit, which, of course, turned out to be identical with Widerõe's flux condition, and the restrictions on the focusing field which would provide radial focusing forces. Walton obtained the correct results and thus solved one of the most subtle problems in induction acceleration—the assurance of radial stabilizing forces. He treated the problem with generality by considering the field requirements of both static and time-varying magnetic and electrostatic fields simultaneously applied to the electrons. Walton showed theoretically that it was possible to arrange the various fields so that radial oscillation about a fixed orbit could be achieved; that is, an electron travelling near the equilibrium orbit can experience forces directing it toward the equilibrium orbit if the fields are adjusted correctly.

Walton made two experiments, one with a static magnetic field superimposed on the rising magnetic field, and another with a rising radial electric field in addition to the rising magnetic field. It is interesting to apply Walton's stability conditions to a Widerõe type of machine in which there is only the time-varying magnetic field. According to Walton's notation, $A_1 > 0$ if $r < r_0$, and $A_1 < 0$ if $r > r_0$. In the case of only the varying magnetic field,

$$A_1 = \frac{M^2}{2\pi^2 r^3} - \frac{MH_z}{\pi r i_0},$$

where M is the mutual inductance between an orbit at radius, r, and the current of amplitude, i_0, producing the magnetic field amplitude, H_z.

Substituting $\frac{M}{i_0} = \Phi$, the flux enclosed by the orbit at r, we get $\Phi - 2\pi r^2 H_z > 0$ for $r < r_0$, and $\Phi - 2\pi r^2 H_z < 0$ if $r > r_0$. In words, this means that the flux enclosed by the orbit is greater or is less than $2\pi r^2 H_z$, depending on the electron being within or without the equilibrium orbit at r_0. Other familiar ways of stating the same conditions are that $\Phi = 2\pi r^2 H_z$ at r_0, which is identical with Widerõe's flux condition, and that H does not decrease as rapidly as a $1/r$ function. Or in a more elegant form, since $H = \frac{a}{r} + \frac{\partial a}{\partial r}$ and $\Phi = 2\pi r a$, where a is the circumferential component of the vector potential, we obtain the result that $\frac{\partial a}{\partial r} < 0$ for $r < r_0$ and $\frac{\partial a}{\partial r} > 0$ for $r > r_0$, which means that the vector potential has a minimum at the equilibrium orbit. This fact provides the most convenient way of determining where the equilibrium orbit is in a betatron, since it is merely necessary to find where the circumferential electric field, which is the time derivative of the vector potential, is a minimum.

Walton's condition, $A_2 = 0$, is automatically satisfied for accelerators with only the varying magnetic field, since in this case A_2 is identically 0. His restrictions on A_3 are taken care of by the restrictions on A_1, since $A_3 = -A_1$ in this case.

Walton used a high-frequency magnetic field obtained from an induction coil, and for a source of electrons he merely hung a filament in the acceleration chamber (Figs. 3 and 4). While high-speed injection is not so important with a high-frequency magnetic field as it is with a slowly varying magnetic field, this method of starting the electrons may have handicapped operation. Walton also made no provision for directing the electrons against a target. Although his experimental attempts were not rewarded with success, his solution of the theoretical problem of achieving radial focusing forces contributed greatly to the eventual success of this scheme.

Thus by 1929 the necessary flux condition for an equilibrium orbit and the requirements for the existence of focusing forces, namely the radially decreasing or bulging magnetic field for axial focusing forces and the necessity for the magnetic field to change less rapidly than $1/r$ for the existence of radial focusing forces, were theoretically established and experimentally tried on models. The axial stability condition was successfully incorporated in the cyclotron by Lawrence and Livingston and completely explained by them in 1932. Although the non-iron type of accelerator with a spiralling orbit as tested by Breit and Tuve was well understood and perhaps successful to some extent, there remained one fundamental problem not solved for the case of an accelerator using a fixed equilibrium orbit such as Widerõe and Walton had planned. This was the problem of whether the electrons, when scattered by residual gas, would oscillate about their equilibrium orbit with decreasing amplitude or would oscillate with increasing amplitude and hence strike the wall, where they would be lost.

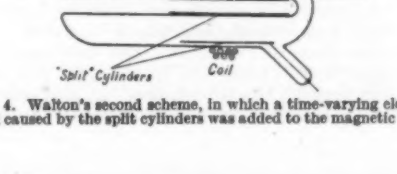

Fig. 4. Walton's second scheme, in which a time-varying electric field caused by the split cylinders was added to the magnetic field.

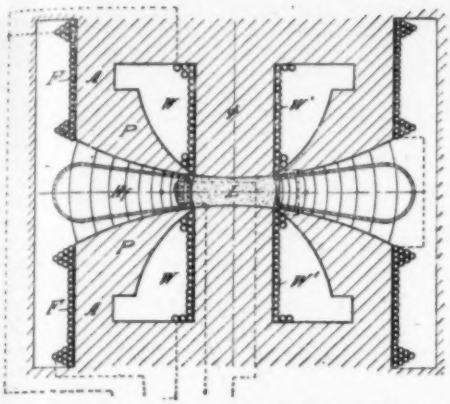

Fig. 5. Steenbeck's apparatus. *P* are the focusing poles which produce a bulging field. A bare filament is suspended within the vacuum tube near the centre.

Because electron paths in an iron core machine are of the order of 100 miles in length, it is very important that such damped focusing exists.

This problem awaited solution until 1939 and 1940, when the complete and rigorous theory of the betatron was developed and tested at the University of Illinois. With the solution to this problem came answers to the question of how to start the electrons so that they would not be scattered to the wall by gas molecules, and how to avoid collision of the electron beam and the electrode structures in the tube on successive revolutions.

In the meantime, several additions were made to the literature. The theory of an accelerator with added electrostatic radial field was examined by Jassinsky in 1936[8]. He suggested the magnetron method of getting the electrons out from a central cathode and into the orbits. Certain instabilities exist in the apparatus which he suggests, such as radial instability due to field shape. However, in spite of the difficulty of trapping the electrons in stable orbits by his system, his is the first suggestion of starting the electron within the region of the magnetic field with an arrangement which gives them a high velocity quickly so that very little low-energy scattering can occur.

In 1937 a United States patent was issued to Steenbeck[7] (applied for in Germany in 1935) on an apparatus of the Widerõe type, using the known stability conditions for flux and field shape. He included methods of disturbing the orbit after acceleration, such as the small coil near the orbit, as Widerõe suggested, and the very practical method of inclusion of a saturable section of the magnetic circuit to disturb the flux distribution and hence the orbit whenever the field becomes large. As in the descriptions of Slepian and Walton, the source of the electrons was to be a simple filament hung in the acceleration chamber; or it was to be a gun, such as Widerõe had used, outside the pole-pieces, shooting into a vacuum tube (Fig. 5).

Although there is no evidence in the patent that a working apparatus is being described, since the essential high-velocity injection and semi-conducting wall coatings as used in modern betatrons are not described, and although the patent gives no evidence that experiments had been tried with the scheme, Steenbeck very recently claimed[6] that in 1935 he had been able to detect a small intensity of 1·8 million electron-volt radiation from this apparatus with a Geiger counter for a detector, but he states that the experiments had been dropped.

Another patent issued to G. W. Penny[9] in 1940 (applied for in 1938) describes an accelerator much like Widerõe's, but in which Widerõe's suggestion for electrostatic focusing electrodes is followed (Fig. 6). He describes the Slepian system of having the magnetic field increase with larger radius to crowd high-speed electrons inward. However, it is likely that the electrostatic focusing electrodes would compensate for this magnetic defocusing only at the smallest of energies, since the magnetic forces increase with the velocity of the electrons and could overpower the electrostatic focusing. However, such electrostatic focusing added to an already stable betatron with the proper magnetic field shape might have value in holding the beam together against space-charge forces at low energies. This would lead to an increase in the amount of charge that could be carried by an orbit and hence to an increased yield.

As already mentioned, the problem of the transient details of the electrons' motion—the increase or decrease of amplitude of oscillation of an electron experiencing the focusing forces—and the problem of how to inject electrons so they do not scatter away and do not hit the starting electrode early in their acceleration, had to wait until 1939 and 1940 for their solution[10]. The rigorous and detailed theory of the motion executed by electrons introduced into a field of a given shape proved that, while the magnetic field is increasing, the amplitudes of oscillation of scattered electrons about their instantaneous circular or equilibrium orbit is decreased or is damped. The amplitude of oscillation is proportional to $1/H^{1/8}$, where H, the magnetic field, increases with time during acceleration. Likewise electrons held in the acceleration chamber after the peak field was reached would soon strike the walls because the magnetic field would then decrease and cause the amplitudes of oscillation to grow.

The usual stability conditions resulted from this theoretical treatment; but the evaluation of the magnitude of the transient motion before an electron reached its equilibrium orbit made it clear at once that it was possible to install a minute electron gun

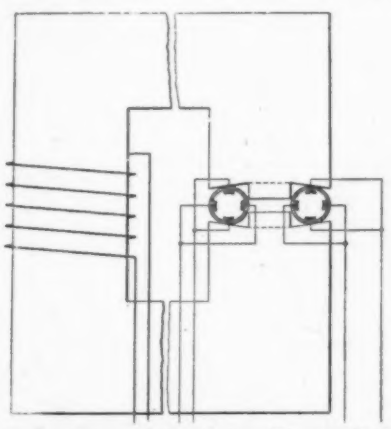

Fig. 6. Penny's suggested accelerator. The magnetic field is supposed to increase as the radius increases. This is shown by the narrower gap at large radii. In place of the magnetic focusing, electrostatic electrodes within the vacuum tube are suggested.

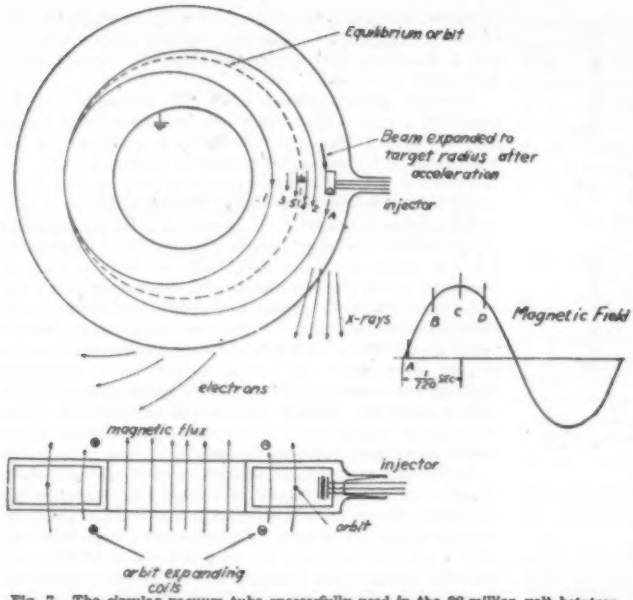

Fig. 7. The circular vacuum tube successfully used in the 22-million volt betatron. Electrons from the injector at *A* proceed to position 1, 2, 3, 4 and 5 in succession as they revolve. Eventually the electrons settle down in the orbit shown by the dotted line. The orbit is expanded at time *C* by sending a current pulse through small wires fastened on the surface of the poles so that additional momentum is given to the electrons.

or injector within the field and close to the equilibrium orbit, so that the transient decrease in amplitude of the motion of the electron about the equilibrium orbit would cause the electron to pass by the injector on successive revolutions without striking the injector. By this method electrons could be started at an energy sufficiently great to avoid much loss of beam intensity by gas scattering.

Although the theory did not include the effect of space charge on the transient motion, it dictated design features which were incorporated into an iron core betatron which proved to be immediately successful when completed on July 15, 1940[11] (Fig. 7). For directing the electrons against a target within the tube, saturation, shading coils, and orbit-expanding coils were used on this first betatron. The initial X-ray intensity from this betatron was equal to that from 100 millicuries of radium, and afterwards this was increased to two or three grams equivalent. This output and the yield of 60 r. per minute at a metre which is attained by the 22-million volt betatrons are clearly large enough for practical applications.

The future will inevitably bring variations and improvements on the established pattern of betatron design. The problem of high-energy electron generation without applying full potential has so far this one accomplished solution; but a great deal of originality and ingenuity will be brought to bear on other solutions as we approach the thousand million electron volt region. However, the efforts made in the future can be no more skilful than some of the published historical attempts and some of the unpublished attempts. In the latter class there are two cases of which I know in some detail that are very worthy of mention.

F. R. Abbott, working under J. E. Henderson at the University of Washington in Seattle, Washington, independently developed Wideröe's flux condition and solved many of the technical problems of fabricating a large iron magnet and glass ring-shaped vacuum tube. He realized the necessity for collecting the electrostatic charge from the wall of the tube, and to do this he inserted a spiral of wire along the inside surface of the tube. He took great care to provide a good vacuum, and he succeeded in creating a very strong 60-cycle magnetic field. Although unsuccessful, his attempt must have been very close to success. He was somewhat handicapped by the rather low-frequency magnetic field.

Another unpublished investigation, which would surely have succeeded were it not for the War in Europe, was that of James L. Tuck at the Clarendon Laboratory, Oxford, who had designed his apparatus in collaboration with L. Szilard. His equipment contained no iron; the field was created by discharging a very high voltage condenser through coils above and below the ring-shaped vacuum tube. Tuck followed Walton's theoretical solution for a means of achieving radial stability, and used large coils above and below the acceleration tube to create the focusing field at the orbit. In addition to these coils, smaller coils near the centre of the vacuum tube added the central flux necessary to hold the equilibrium orbit at a fixed large radius. The field was bulging to provide axial stability. A high-resistance silver coat covered the inside wall of the vacuum tube into which electrons were to be shot from the outside by an electron gun as Breit and Tuve had done. Since a rapidly changing magnetic field capable of giving 30,000 volts per turn was employed, an externally injected electron would gain an energy which is large compared with the injection energy on the first revolution, and hence it would be trapped.

The provision for shifting the orbit to the target was to disturb the flux condition by short-circuiting an auxiliary coil coupled to the focusing field coils by a rectifier which would pass current only when the magnetic field began to decrease. The design of Tuck's machine was such that greater energy would be produced by it than by the design of Breit and Tuve. Of all the attempts to accelerate electrons, before the initial success of the betatron, Tuck's scheme seems

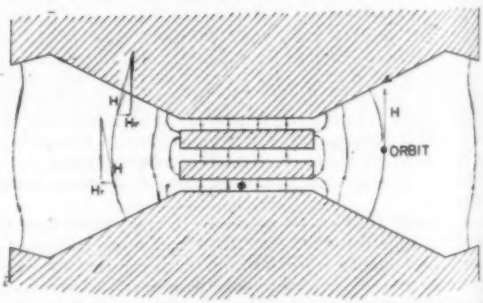

Fig. 8. Approximate shape of the poles required for the 22-million volt betatron. The rims of the poles are turned inward to contract the rapidly decreasing field at the corner of the pole.

to have been most promising and most complete in technical detail. How many other investigators have come as close to success with the problem, but have been interrupted by war-work or other circumstances, we may never know.

[1] *Phys. Rev.*, **43**, 217 (1933).
[2] U.S. Patent 1,645,304.
[3] Carnegie Year Book, No. 27, 209 (1927–28).
[4] *Arch. Electrotech.*, **2**, 400 (1928).
[5] *Proc. Camb. Phil. Soc.*, **25**, 469, part 4 (Oct. 1929).
[6] Jassinsky, W. W., *Arch. Electrotech.*, **30**, 500 (1936).
[7] Steenbeck, M., U.S. Patent 2,103,303.
[8] Steenbeck, M., *Phys. Z.* (Feb. 1943).
[9] Penny, G. W., U.S. Patent 2,193,602.
[10] Kerst, D. W., and Serber, R., *Phys. Rev.*, **60**, 53 (1941).
[11] Kerst, D. W., *Phys. Rev.*, **60**, 47 (July 1, 1941).

MICROBIOLOGY, BIOCHEMISTRY, AND THE GENETICS OF MICRO-ORGANISMS

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In the last few years certain important advances have been made in the genetics of micro-organisms. Their implications are fundamental for genetics and go far beyond it, since they forecast a synthesis with microbiology, immunology and biochemistry. It is therefore fortunate that a very suggestive view of the whole field is now available in the form of a special issue of the *Annals of the Missouri Botanical Garden**, containing the papers read, and the ensuing discussion, at a conference on "Gene Action in Micro-organisms" held last February at St. Louis. The volume should be read by all those interested in the general trends of biology.

For the study of heredity and variation in higher organisms the geneticist has had to avoid many pitfalls, some of which are of everyday occurrence also with micro-organisms. For example, the microbiologist has often to decide whether a change in a strain is a direct consequence of certain external conditions or of selection under such conditions. Collaboration with the geneticist can here be very fruitful, as shown in the papers by Demerec, Delbrück, Spiegelman, Luria, Hollaender and Gowen at the Conference. But the advantage is reciprocal, since the microbiologist holds in store an impressive mass of facts having direct bearing on genetics. He can, for example, point out that in most groups of Fungi (and, I would suggest, probably in Protozoa with plasmodial stages) genetic systems of an entirely unexplored kind are the rule. They are based on 'heterokaryosis', instead of heterozygosis; that is, on the presence of genetically different nuclei in a multinucleate cell, instead of genetically different sets of chromosomes in the diploid, or polyploid, nucleus of a uninucleate cell.

The recent developments in the genetics of micro-organisms have led to important advances in directions other than heredity and variation. A fascinating field is that opened up with the attempt by Sturtevant and Emerson to introduce immunological concepts and techniques in the study of the nature of genes. The mode of inheritance of cell antigens in vertebrates prompted Haldane to suggest long ago that there might be a structural relationship between gene and antigen. On this basis Sturtevant has raised the question of whether antibodies against natural antigens might induce *specific* mutations in the corresponding genes, an idea that Hindle had independently suggested to me. A preliminary experimental attempt by Emerson with the mould *Neurospora* has given encouraging, but by no means adequate, results. In the meantime, Emerson has made an interesting formal analysis of the possible structural relationships between gene and gene-product which considers gene reproduction and gene action as two complementary aspects of the same activity, somewhat like a key moulding its key-hole and the key-hole moulding the key. Clearly, if results are forthcoming, one of the most spectacular advances in genetics is 'round the corner'.

Another important field in which progress has been impressive is that of the genetic control of metabolic reactions. Up to five years ago, in very few cases had the action of genes been traced right down to the identification of elementary biochemical steps controlled by genes. The best-known example was that of the genetics of flower pigments. The work on the mould *Neurospora* by Beadle, Tatum and their associates at Stanford University has now added an impressive mass of far more complete examples. The general conclusion to which they have come is that each step in a chain of metabolic reactions is controlled by a specific gene. Since genes that have mutated, especially as a consequence of X-ray treatment, are often inactive, it is possible to block a metabolic step by producing a mutation in the corresponding gene. Much to the advantage of the biochemist, it follows that in a chain of metabolic reactions with an unknown number of steps, it is possible to identify the single steps and reconstruct the chain by identifying the genes involved.

There seems to be little doubt that the control by genes of elementary biochemical steps takes place through enzymes. This raises the question of the relations between gene and enzyme. The study of the inheritance of enzymes in yeast, started by Winge and developed by Lindegren and Spiegelman, suggests that the production, or better the specificity, of each enzyme is under the immediate control of a gene. We must, however, be cautious on this point, since so far the evidence only shows that the ability or inability to produce an enzyme is generally inherited as a Mendelian unit. It is a reasonable inference on general grounds—but nothing more than an inference—that, of two allelomorphs of a gene, one may determine the production of one enzyme and the other that of a different enzyme. Clearly, this must be settled experimentally as soon as possible.

Granted that genes, or at least some genes, control enzyme specificity, it seems to me that we have to find a way out of the following difficulty. The different enzymes of a cell, each under the immediate control of a specific gene, are produced at enormously

* *Ann. Missouri Bot. Garden*, **2**, No. 2 (1945). Conference on "Gene Action in Microorganisms".
Lindegren, C. C., "Mendelian and Cytoplasmic Inheritance in Yeasts."
Tatum, E. L., and Beadle, G. W., "Biochemical Genetics of Neurospora."
Demerec, M., "Genetic Aspects of Changes in *Staphylococcus aureus* Producing Strains Resistant to Various Concentrations of Penicillin."
Spiegelman, S., "The Physiology and Genetic Significance of Enzymatic Adaptation."
Hollaender, A., "The Mechanism of Radiation Effects and the Use of Radiation for the Production of Mutations with Improved Fermentation."
Greenstein, J. P., and Chalkley, H. W., "The Influence of Nucleic Acid on Dehydrogenase Systems."
Gowen, J. W., "Genetic Aspects of Virulence in Bacteria and Viruses."
Sonneborn, T. M., "Gene Action in *Paramecium*."
Delbrück, M., "Spontaneous Mutations of Bacteria."
Luria, S. E., "Genetics of Bacterium-Bacterial Virus Relationship."
Emerson, S., "Genetics as a Tool for Studying Gene Structure."
Discussion.

different rates; this is also true of the same enzyme in different cells of a multicellular organism. What is known of mitosis, on the other hand, suggests that all genes in a nucleus reproduce at the same rate. Unless gene reproduction and gene action are totally independent of each other, we have to reconcile the uniformity in reproduction of the genes with the enormous variation in the output of what we believe to be their primary products. This is, of course, one way of looking at the problem of cell differentiation. Two series of important investigations may suggest a way out of this difficulty.

First, there are the findings of Spiegelman and Lindegren in yeast. A dominant gene is necessary for yeast cells to produce the 'adaptive' enzyme melibiozymase. In cells carrying the gene, the enzyme increases in amount with an autocatalytic trend when they are put in a medium containing melibiose; it disappears *reversibly* in a few hours when melibiose is removed. With appropriate crosses, cells can be obtained lacking the gene but carrying some melibiozymase in the cytoplasm: these cells can go on producing melibiozymase indefinitely, and for an indefinite number of generations, if kept in contact with melibiose; removal of melibiose brings about the *irreversible* loss of the enzyme.

Secondly, there is Sonneborn's remarkable work on *Paramecium*, only the essentials of which can be referred to here. Certain strains carry in the cytoplasm a 'kappa' substance which, released into the water, poisons it for other strains. With appropriate crosses, individuals can be obtained with the same genetic constitution as the former but lacking the 'kappa' substance in the cytoplasm. These individuals are unable to produce the 'kappa' substance unless some of it is introduced into the cytoplasm by means of special devices. The production of 'kappa' can also be blocked by suitable gene substitutions. In other words, 'kappa' is produced if some of it is present in the cytoplasm to start with and the genetic constitution is appropriate.

From these investigations and the stimulating discussion, especially by Muller, at the St. Louis Conference, I would like to draw the following very speculative picture. Enzymes, or similar proteins, occur in the cytoplasm, and can increase in amount autocatalytically if a trace is present to begin with. For this increase specific precursors, indirectly controlled by genes, are required. Furthermore, certain of these autocatalytic proteins, being unstable, can be maintained and increase in the cytoplasm only if specific substrates, intra- or extra-cellular, are available. Lastly, while those which are stable, or stabilized by ever-present intracellular substrates, can perpetuate themselves from cell to cell simply through cytoplasmic transmission, those which require a stabilizing extracellular substrate would be continually liable to be lost. There is, however, a safety mechanism: a specific gene keeps a trace of these unstable substances always ready; whenever the external substrate is provided, this trace can start the autocatalytic process independently of the specific gene. What the gene has to do is therefore only to synthesize an initial amount of the substance. Until we can give each its proper chemical status, there is no harm in lumping all these 'self-reproducing' substances, gene-initiated or otherwise, under the name of 'plasmagenes'. Those that can be transmitted externally from one cell to another, and there cause disease, may be viruses, as Darlington suggests. It is noteworthy that pathologists, starting from the virus end of the story, have long held more or less the same view.

Should the preceding picture be broadly correct, the large differences in output of different enzymes in a cell, and of the same enzyme in different cells, would be reconcilable with the view that most enzymes are primary gene products. We may conceive that all genes in a cell are, at any one time, working all at the same rate, say producing the same number of molecules of their different primary products; yet the actual amount of each in the cytoplasm will vary secondarily, depending, among other things, on its ability to increase autocatalytically, on the limiting amount of specific precursor, and on extra- or intracellular substrates. Since the presence of precursors and of intracellular substrates depends on the previous history of the cell, as pointed out by Spiegelman and Lindegren, there is here a basis for cell differentiation in time, each successively available precursor or substrate making possible the independent autocatalytic increase, and the activity, of new gene-initiated plasmagenes.

On the whole, the nucleus seems to emerge from these studies with enhanced prestige. Its control of the activities of the cell seems to be largely a one-way affair. No doubt the nuclear membrane holds a good deal of the secret, and it would be desirable to start tackling it.

INTERNATIONAL SCIENTIFIC CO-OPERATION

By Prof. F. J. M. STRATTON, O.B.E.
General Secretary of the International Council of Scientific Unions

THE gradual easing of restrictions on travel and on the general release of scientific discoveries has made it practicable once more to arrange international meetings of men of science. The general assembly of the International Council of Scientific Unions will meet in London during July 22–24, 1946. Meanwhile its Executive Committee has held a meeting in London attended by representatives of most of the International Unions adhering to the Council.

In welcoming the Committee at its opening session, Prof. A. V. Hill, foreign secretary of the Royal Society, pointed out that science in its own interest must remain an international concern and that the future of civilization itself depends upon the close co-operation of scientific men throughout the world. Taking as an example the field of medicine and the high ethical principles which have been developed therein, he emphasized the need for a common standard of ethical behaviour in science. "In standards of truthfulness and integrity, in readiness to co-operate with other scientists of good will everywhere, and in refusal to use the common knowledge, prestige and power of science for base, harmful and selfish ends, scientists should feel themselves bound by an honourable and unbreakable obligation".

The heavy toll of recent years on those prominent in the field of international science was revealed by the list of members of the Committee who had died since its last meeting: Marconi and Joji Sakurai, vice-presidents; Sir Arthur Eddington, president of the International Astronomical Union; Prof. Nicola

Parravano, president of the International Union of Chemistry; Sir Albert Seward, president of the International Union of Biology; Dr. Philippson, general secretary of the International Union of Physics; and General Bourgeois, former vice-president, representing the International Union of Geography.

The president of the Council, Prof. C. Fabry, had been compelled to resign on account of ill-health (he has since died, December 11), and Dr. H. R. Kruyt was elected president; Dr. Elmer D. Merrill being elected vice-president in succession to Dr. Kruyt.

The International Council, or rather its predecessor the International Research Council, was created after the First World War and took as its main task the creation of the various international scientific unions. In the early days the Council exercised a certain amount of control over these unions in such questions as membership and finances. As the individual unions became established and strong, this control proved irksome, and a change took place in the relative positions of the Council and the unions, largely the work of the then general secretary of the Council, the late Sir Henry Lyons. Now it would be fairer to say that the unions control the Council, the majority of the members of the Executive Committee being the representatives of the unions. The officers and two members of the Executive Committee only are appointed by the General Assembly, at which the national adhering organisations have a preponderating vote.

The international unions meet in general assembly as a rule once in three years for discussions on the field of knowledge lying within their purview; by means of numerous committees of specialists work is carried on between the meetings and is reported on and discussed at the meetings of the general assembly. Ninety-two such committees have been reported as working in the various international unions, their activities covering a very wide field of knowledge. Each union also appoints at its general assembly a small bureau of officers who are able to answer for the union as a whole between meetings. Some of these have been badly broken up during the war years and are only now being rebuilt.

In addition to the 'international unions', there are in existence a number of 'international congresses' which meet from time to time in different countries but have no continuity of active existence between meetings. Broadly speaking, it would be safe to say that those who have been actively concerned with these congresses and have pleasant recollections of their scientific discussions and fellowship do not desire to change them into international unions. The weakness of their position, however, is the lack of a body between meetings that can answer for the subject covered by the congress and that can nominate the most suitable members for co-operation with unions desiring their help. Border-line subjects needing international study are catered for by joint commissions between the various unions. It would be a help if an international congress would appoint a bureau, to function between meetings, to which the various requests for assistance or advice could be addressed. This is done in some cases already. In other cases union and congress both exist. The general assembly of the union meets at the same place as the congress and within a day or two of the congress meeting. Elasticity of organisation is desirable to meet the different needs and outlook of workers in different fields.

Joint commissions of the unions have been active for more than twenty years in some cases; for example, the joint commissions on latitude variation, on the determination of longitudes by wireless and on the Bureau de l'heure, in which both the Unions of Astronomy and of Geodesy and Geophysics are concerned. Some joint commissions are only planned as yet; for example, a commission on ionospheric research which it is hoped to set up in due course with members from the Unions of Astronomy, Geodesy and Geophysics, Radio-Science and Physics. Other joint commissions proposed at the recent meeting of the executive committee of the Council are on oceanography (Biology and Geodesy and Geophysics), physico-chemical constants (Physics and Chemistry), viscosity (Chemistry, Biology, Geodesy and Geophysics). In the work of this latter commission it is hoped that means will be found to secure the co-operation of the International Congress of Applied Mechanics.

In addition to joint commissions between the unions, which the Council may help to arrange, the Council also appoints committees of its own such as the Committee on Solar and Terrestrial Relationship, in which are members from the Unions of Astronomy and of Geodesy and Geophysics. Another committee of the Council is one on science and social relations (C.S.S.R.) appointed in 1937 to survey the most important results obtained and the directions of progress that are opening and of points of view brought forward in the physical, chemical and biological sciences, with reference to their interconnexion, the development of the scientific picture of the world and their practical application in the life of the community. In response to the inspiring and untiring energy of Prof. J. M. Burgers, secretary of this Committee, reports were being received from countries and unions in the years 1938–39 in readiness for collation and publication, when the outbreak of war interrupted the work; some of these reports, such as one on the social aspects of science in Estonia, have become of historical interest only. The Committee has now been asked to prepare a survey of the published views of scientific bodies on the problems now arising in connexion with researches into such matters as nuclear physics and drugs. Such a survey may enable the Council to make a reasoned pronouncement, supported by the full weight of international science, on the ethical problems which face scientific men in particular to-day, thus accepting the challenge offered by Prof. Hill to the Executive Committee in his address of welcome at its recent meeting. A valuable contribution to the work of the C.S.S.R. will be provided by the annual reports on the work of the various unions, which the Council hopes to receive in future and to distribute widely.

The Council is linked by a convention to the International Organisation for Intellectual Co-operation of the League of Nations; the convention defines the separate spheres of action of the two bodies and the various ways in which each can help the other. While the fate of the International Organisation of the League of Nations is uncertain at present, there has come into existence the United Nations Educational, Scientific and Cultural Organisation. At the recent conference at which this body was formed, a resolution was adopted, on the proposal of the delegation from the United States, asking the preparatory commission to invite its executive committee to examine with the International Council of Scientific Unions such methods

of collaboration as might be practicable to strengthen the programmes of the two organisations within the limits of their joint concern; the Executive Committee of the Council has appointed a committee to consult with representatives of the new Organisation, and it is hoped that much may arise from their discussion to the mutual advantages of both bodies. The clause introducing this resolution may well be taken as defining the task now facing the International Council of Scientific Unions:

"Il est urgent de reconstituer et de développer les moyens permettant aux savants de tous les pays d'échanger des renseignements et de travailler en commun au développement de la science et à son utilisation au service de l'humanité".

OBITUARIES

Andrei Petrovich Semenov-Tian-Shanski

THE whole development of insect taxonomy and biogeography in Russia during the last half-century has been most closely connected with the name of A. P. Semenov-Tian-Shanski, whose death from pneumonia during the blockade of Leningrad in 1942 has now been reported.

Andrei Petrovich was born on June 9, 1866, the son of the famous geographer, Peter Semenov, who was later granted the addition Tian-Shanski to his name as a tribute to his exploration of the Central Asian ranges. The father's love of Nature and serious interest in its study were inherited by the son, who published his first scientific paper while still a student at the University of St. Petersburg, from which he graduated in 1889. This paper was the forerunner of a long series (more than 250) of works on entomology, many of them monographic in character. The official career of Andrei Petrovich was brief, as he was on the scientific staff of the Zoological Museum of the Academy of Sciences only for a short period (1890–96) and then again some years before his death. As he had, however, considerable private means, he was able to devote himself entirely to his scientific work.

Semenov's main interest was the taxonomy of the Coleoptera, but no less outstanding were his contributions to the systematics of the Hymenoptera (particularly Chrysididæ and Siricidæ) and the Dermaptera. His renown as a taxonomist was not due merely to the large number of descriptions (some eight hundred new species, about a hundred new genera and one new family), but also to their unique precision, combined with a profound critical appreciation of the value of morphological characters, which he never studied on a 'type' but always on a large series of specimens. Theoretical problems of taxonomy attracted him strongly, and in 1910 he produced a paper on the limits of species and of lower taxonomic categories, where morphological, phylogenetic and biogeographical criteria were used to define the categories and to represent them as phases of a dynamic system of organisms in process of evolution.

Biogeographical and evolutionary aspects always loomed largely in Semenov's taxonomic work, and many of his masterful monographs have a much wider interest than for specialists in the particular group. His contributions to the zoogeography of Central Asia are particularly noteworthy, and his crowning achievement was a zoogeographical map of the Palæarctic Region published in 1936.

As a president of the Russian Entomological Society for most of his life; joint founder of the *Revue Russe d'Entomologie*; initiator and most active member of the Biogeographical Commission of the Russian Geographical Society; and one of the most prolific writers on entomology, Semenov had a profound influence on the development of Russian entomology. A large number of insect taxonomists and biogeographers now working are proud to call themselves his pupils, though he never taught except by example and by incisive criticism of published papers in his many reviews. He was also a most conscientious correspondent, and I was one of the many who derived great benefit from his thorough, and often very outspoken, discussions of scientific problems submitted to him.

Most entomologists tend to be one-sided specialists, but A. P. Semenov, with his aristocratic, highly cultured background, was anything but narrow in his interests. Protection of Nature; organization of museums; geographical exploration; game laws—such was the range covered by his writings in the technical and general Press. A problem that particularly attracted his attention was that of the Russian Navy, and he was even elected in 1914 the president of the Russian Naval League. His most serious hobby, however, was the study of the classics, and he specialized in verse translations of Horace. His knowledge of classical languages found its use in diagnoses of insects, which are written not in the usual 'entomological Latin', but in a Latin which is beyond most entomologists. In the later years of his life, when he had almost lost his sight, he concentrated on the theory of verse, and wrote a book on Pushkin's life and poetry.

Although he travelled abroad only once, when young, Semenov-Tian-Shanski was probably the best-known Russian entomologist in every country of the world. He was an honorary fellow of the Royal Entomological Society of London and of the Prague Entomological Society; life-member of the Société Entomologique de France; and of course honorary member of a score of Russian entomological and natural history societies.

Vast private entomological collections (more than 700,000 specimens, including hundreds of types), library and archives were all presented by Semenov during his life to the Academy of Sciences.

B. P. UVAROV.

Mr. Alfred Lucas, O.B.E.

THE death of Mr. Alfred Lucas at Luxor on December 10, at the age of seventy-six, marks the close of many years of useful service to chemistry and archæology. His scientific career began with the analysis of foods and drugs at the Government Laboratory, London; but persistent ill-health forced him, after several years, to withdraw to the warmer climate of Egypt. Recovered somewhat, he joined the Egyptian Government service and in time became responsible for the Survey Department Laboratory which, under his able direction, developed beyond recognition. From a concern devoted primarily to the analysis of minerals, it became the Chemical Department of the Ministry of Finance, embodying the Assay Office and the petroleum refinery at Suez. By the routine testing of materials in this laboratory, considerable economies were

effected and many problems of national importance were made the subject of special study: such contributions were of the utmost value during the First World War and for this work Mr. Lucas was awarded the O.B.E. A few years later he retired on pension. The fruits of these various experiences form the material for his "Forensic Chemistry", now in its third edition.

Egypt became the land of Lucas's adoption, and Egyptology the main hobby of his retirement. A stimulus was given to these studies by his association, as official chemical adviser, with Dr. Howard Carter at the opening of the tomb of Tut-ankh-Amûn; and his contributions on the "Chemistry of the Tomb" in vols. 2 and 3 of Carter's book ("The Tomb of Tut-ankh-Amen") are of outstanding interest and lasting importance.

Lucas has done much by example and precept to establish safe laboratory methods for the treatment of antiquities. His book on "Antiques, their Restoration and Preservation" is deservedly well known, and his "Ancient Egyptian Materials and Industries" is already a classic.

His life was a triumph over illness and circumstances; it left him with a deep sympathy for his fellow-men, and with a quiet but keen sense of humour which was the delight of his friends.

H. J. PLENDERLEITH.

NEWS and VIEWS

Royal Astronomical Society: Award of Gold Medal

THE Gold Medal of the Royal Astronomical Society has been awarded to Prof. J. H. Oort, for his investigations on the dynamics and rotation of the galactic system. The hypothesis of the rotation of the galactic system about a point near the centre of the system of globular clusters was suggested by Lindblad in 1925 as a possible explanation of the systematic differences of the motions of the globular clusters and R. R. Lyræ variables from those of the brighter stars in our neighbourhood. Prof. Oort provided the first direct observational evidence that the galactic system was rotating. For this purpose he employed several groups of stars of high luminosity and analysed both their radial velocities and proper motions. A series of brilliant investigations fully established the rotation of the galaxy, provided estimates of the distance of the centre of rotation, of the velocity of rotation at the sun's distance, and of the total mass of the system. Important conclusions about the distribution of mass and the absorption of light in the galactic plane were obtained. Determinations were also made of secular parallaxes and of the precession and motions of the equinox. Investigations of the dynamics of the galactic system established that both star-streaming and the systematic motions of stars of high velocity can be considered as steady phenomena. Prof. Oort has made many other contributions of importance to astronomy. During the German occupation of the Netherlands, Prof. Oort resigned his professorship, along with most of the other professors of the University of Leyden, rather than submit to Nazi dictation. He has recently been appointed director of the Leyden Observatory. Since 1935 he has been general secretary of the International Astronomical Union.

Institution of Electrical Engineers Awards: Honorary Membership

THE Earl of Mount Edgcumbe has been elected an honorary member of the Institution of Electrical Engineers, in recognition of his distinguished work as an electrical engineer, and of the services rendered by him to the Institution. Lord Mount Edgcumbe, who is a fellow of University College, London, served his pupillage with Crompton and Co. at Chelmsford, and in 1897 joined the staff at Northampton Polytechnic Institute in London. In 1900 he founded the firm of Everett Edgcumbe and Co., Ltd., of which he is chairman and managing director. He has been associated with the Institution of Electrical Engineers since 1897, and was president during 1928–29. He is the author of a number of papers read before the Institution and published in its journal, for which he has received several premiums, including the Ayrton Premium and the Silvanus Thompson Premium. Lord Mount Edgcumbe has also contributed valuable assistance in the international field, particularly in his capacity, from 1927, as honorary secretary of the International Electrotechnical Commission. He is also a vice-president of the International Commission on Illumination, vice-president of the British Electrical and Allied Manufacturers' Association and a past president of the Illuminating Engineering Society of Great Britain.

Faraday Medal

SIR EDWARD APPLETON has been awarded the Faraday Medal of the Institution of Electrical Engineers, for the conspicuous services rendered by him in the advancement of electrical science, particularly in the field of radio propagation. This Medal is awarded not more frequently than once a year, either for notable scientific or industrial achievement in electrical engineering or for conspicuous service rendered to the advancement of electrical science, without restriction as regards nationality, country of residence or membership of the Institution. Educated at Bradford, and then at St. John's College, Cambridge (scholar and exhibitioner), Sir Edward became, in 1920, assistant demonstrator in experimental physics at the Cavendish Laboratory. He was Wheatstone professor of physics, University of London, during 1924–36 and Jacksonian professor of natural philosophy, Cambridge, during 1936–39. His early researches in connexion with the propagation of radio waves, and particularly on their reflexion and refraction, have been acknowledged by naming one of the ionospheric layers after him. His work was of fundamental importance in the development of radiolocation. In his capacity of secretary of the Department of Scientific and Industrial Research, which he has filled since 1939, his inspiration and guidance of the use of science in many fields of national activity have been major factors in the success of the scientific effort of Great Britain. Sir Edward, who was elected to the Royal Society in 1927, is chairman of the British National Committee for Radio-telegraphy, vice-president of the American Institute of Radio Engineers and president of the International Scientific Radio Union.

Natural History at Aberdeen : Prof. V. C. Wynne-Edwards

Prof. V. C. Wynne-Edwards, who has been appointed to succeed Prof. A. C. Hardy in the regius chair of natural history in the University of Aberdeen, will bring a fresh point of view to the study of zoology in Great Britain. He is primarily a naturalist, but having trained in Oxford under the late Prof. E. S. Goodrich, he has a full understanding of the problems of general zoology. After taking his degree he worked for some time at Plymouth, mainly on herrings, and then for a while at the University of Bristol, but his reputation has mainly been made in Canada, in the Department of Zoology of McGill University. His earlier interest was in birds, and while in Britain he published papers on the behaviour of starlings, night-jars and other subjects. For several years he made a careful study of the habits and migrations of sea birds, and for this purpose once made no fewer than eight trans-Atlantic crossings in one summer. In Canada, his work has extended to the whole fauna and flora especially of the St. Lawrence region, and he has demonstrated that the groups of arctic plants found in this area cannot be relics of areas left unglaciated during the last ice-age. His great power of observation and ability to stimulate that of others will provide a refreshing new influence in Britain. We may hope that he will show the zoologists of this country, as he has those of Canada, how much can be seen by anyone who has the energy to move about in the country.

Geological Society of London : Awards

The following awards have recently been made by the Geological Society: *Wollaston Medal* to M. Emmanuel de Margerie, honorary professor in the University of Strasbourg and formerly head of the Service géologique d'Alsace et de Lorraine, in recognition of his researches "on the Mineral Structure of the Earth", especially in the Jura Mountains of France, and for his bibliographical work; *Murchison Medal* to Dr. Leonard Hawkes, head of the Geological Department of Bedford College and reader in geology in the University of London, for his researches on Icelandic geology and on rocks in the glassy state; *Lyell Medal* to Dr. R. H. Rastall, for his researches into the petrology and structure of the Mesozoic rocks of eastern England and for his editorship of the *Geological Magazine*; *Wollaston Fund* to Mr. James L. Begg, of Glasgow, for his studies on the geology and palæontology of the Lower Palæozoic rocks of the Girvan district; *Murchison Fund* to Dr. Alan Wood, demonstrator in geology in the Imperial College of Science and Technology, for his palæontological researches, notably on Carboniferous faunas and the evolution of the Foraminifera. *Lyell Fund:* one moiety to Dr. William Pulfrey, of the Mining and Geological Department, Kenya Colony, for his work on Palæozoic sediments of North Wales and on the Kavirondo area of Kenya Colony; another moiety to Mr. Percy T. Cox, for his researches on the Cretaceous Foraminifera and on the stratigraphy of the Persian Gulf.

Naphthalene and the Dye Industry in Britain

During the ten years 1886–96, the late Prof. H. E. Armstrong and Prof. W. P. Wynne were engaged, at the Central Technical College, London, in a series of researches which served to establish on a sure basis the structure of the large number of new naphthalene derivatives which were at that time being produced by empirical methods and were attracting the attention of chemists in the rapidly developing dye industry. Armstrong and Wynne completed the work of identification of all of the isomeric di- and tri-chloronaphthalenes, establishing the orientation of the chlorine atoms in the naphthalene ring for every one of them. These compounds have been used as reference compounds for the establishment of the orientation of other naphthalene derivatives of unknown structure. This work was, of course, fundamental for naphthalene chemistry and has come to be regarded as classic. There can, however, be few classical chemical researches which have been so inadequately reported in the literature as this work of Armstrong and Wynne. No connected account of it exists, the results of the work being recorded only in the briefest manner in the form of summaries of verbal communications to the Chemical Society printed in the *Proceedings* of that Society, and in the *Chemical News*.

Fortunately, however, a record of a different kind is available, for Prof. Wynne has preserved with meticulous care samples of the compounds prepared in the course of the research. Thus the actual samples of all the di- and tri-chloronaphthalenes, prepared, most of them, by several different routes, are available, in addition to the intermediate products of all kinds, such as chloronaphthols, mono-, di- and tri-chloronaphthalene sulphonic acids and chlorides, naphthylamine sulphonic acids, and many others which were made in the course of the work. During the years of his retirement, working in the University of Cambridge Chemical Laboratories, Prof. Wynne has given much attention to this collection, filling in gaps and perfecting it. This beautiful collection of some 465 specimens has now been presented by Prof. Wynne to the Dyestuffs Division of Imperial Chemical Industries, Ltd. The gift is more than a museum piece of historical interest, or even a collection of reference samples; it is a constant reminder of the debt which industry owes to the work of disinterested academic scientific workers. It has been accepted by the Company with the greatest pleasure and gratitude, and is to occupy a place of honour in the Research Department.

ASLIB Microfilm Service

To meet war-time difficulties, the Association of Special Libraries and Information Bureaux, with the help of the Royal Society, the Rockefeller Foundation and British industry, organised a microfilm service which started operations in April 1942 and which supplied, in microfilm or paper enlargements, copies of scientific and technical periodicals published in Germany and countries occupied by Germany during the war years. The service was used by the British and American Governments, by more than three hundred scientific institutions and research stations in Great Britain and the Empire and, under a special agency arrangement, by several hundred institutions in the United States. The library of master negatives accumulated for the purpose contains some 14,000 separate issues of approximately 500 different titles, equivalent to a total of more than a million pages. On behalf of the British Council, microfilm copies of British periodicals were prepared for China when that country was cut off from ordinary surface transport; this work grew, until in 1945 some 170 periodicals were regularly microfilmed and six positive copies of each were dispatched to China by air.

The end of the War reduced the demand for the special functions the ASLIB Microfilm Service had been set up to fulfil, and by arrangement with the Royal Society of Medicine, it has become an integral part of the Society's Central Medical Library Bureau (see *Nature*, January 12, p. 44). It is hoped, nevertheless, to be able to continue to serve non-medical demands for documentary reproduction. Requests for copies from existing master negatives may be sent to the old address of the ASLIB Microfilm Service at the Victoria and Albert Museum, London, S.W.7, until January 31. After that date all orders, including requests for copies of individual references from varied literature, should be addressed to the Association of Special Libraries and Information Bureaux, 52 Bloomsbury Street, London, W.C.1.

Royal Commission for the Exhibition of 1851

THE Commissioners for the Exhibition of 1851 have decided to re-commence the awards of senior studentships and overseas scholarships for scientific research. The modified regulations provide for the award of four studentships of £500 a year each, tenable for two years. Overseas scholarships are worth £350 a year for two years; ten are being offered distributed as follows: Canada, 3; Australia and India, 2 each; South Africa, New Zealand and Eire, 1 each. The age limits for senior studentships and for scholarships are thirty and twenty-six respectively. Applications must reach the office of the Commissioners by May 1 (from South Africa, Eire and India), and June 1 (Canada, Australia and New Zealand).

The Secretary of the Royal Commission for the Exhibition of 1851 is preparing a new edition of the Commission's "List of Whole-time Research Awards for Scientific Research, other than Professorships, offered by Public and Private Bodies in Great Britain and Northern Ireland". He would be glad to receive at 1 Lowther Gardens, Exhibition Road, London, S.W.7, for inclusion in the revised list particulars of any new awards (of value not less than £150) created since November 1937 and of any alterations made since that date in the value or conditions of existing awards. The list includes: (*a*) open awards offered by private and public bodies other than universities or colleges, (*b*) open awards offered by universities or colleges, (*c*) awards restricted to candidates from particular localities or institutions. It does not include awards offered by universities and scientific societies exclusively for the benefit of their own members nor scholarships for which British subjects are not eligible. It does contain, however, some of the more important fellowships which are available for British subjects although administered by foreign foundations.

Conference on Industry and Research

A TWO-DAY "Conference on Industry and Research", under the auspices of the Federation of British Industries, will be held at the Kingsway Hall, London, on March 27 and 28. The Conference will take as its leading subject the application of science by industry and the part that research is playing, and can play, in promoting industrial efficiency, exports, full employment and a higher standard of living. It will be divided into four sessions as follows: (1) Science, industry and the community (speakers, Sir William Larke, Sir Edward Appleton, Sir Harold Hartley and Sir Ernest Simon); (2) scientific research and production (speakers, Dr. J. R. Hosking (Paints Div., I.C.I., Ltd.), Mr. A. Healey (Dunlop and Co., Ltd.); Dr. C. C. Paterson (General Electric Co., Ltd.)); (3) scientific research and industrial expansion (speakers, Dr. R. E. Slade (lately research controller, I.C.I., Ltd.), Mr. C. H. Davy (Babcock and Wilcox, Ltd.), Mr. A. J. Philpot (British Scientific Instrument Research Association)); (4) application of research in industry (speakers, Dr. P. Dunsheath (Henley's, W. T. Telegraph Works, Ltd.) and Sir Raymond Street (chairman of the Cotton Board)). Parts of sessions 2, 3 and 4 will be open to discussion; Sir William Larke will sum up at the end of the Conference.

The Night Sky in February

NEW moon occurs on Feb. 2d. 04h. 43m., U.T., and full moon on Feb. 16d. 04h. 28m. The following conjunctions with the moon take place: Feb. 13d. 03h., Mars 3° N.; Feb. 13d. 09h., Saturn 2° S.; Feb. 20d. 20h., Jupiter 3° S. Occultations of stars not fainter than magnitude 6 are as follows: Feb. 9d. 20h. 33·2m., 148 B. Taur. (*D*); Feb. 12d. 19h. 03·3m., 87 B. Gemi. (*D*); Feb. 23d. 03h. 07·0m., 34 Libr. (*R*); Feb. 23d. 04h. 36·5m., ζ Libr. (*R*). The times are for Greenwich, and (*D*) and (*R*) refer to disappearance and reappearance respectively. Mercury, in superior conjunction with the sun on Feb. 11, is not well placed for observation early in the month, but as the planet sets on Feb. 28 about 1¼ hour after the sun, it can be seen in the western sky towards the end of the month. Venus is in superior conjunction with the sun on Feb. 1, and is not very well placed for observation during the month except towards the end, when it sets about 25 minutes after the sun and can be seen in the western sky. Mars, in the constellation of Gemini, is visible throughout the greater portion of the night and does not set before 6h. in the middle of the month. On Feb. 22 Mars is stationary. Jupiter, in the constellation of Virgo, cannot be seen before midnight early in the month, but is more favourably placed for observation later: the times of rising at the beginning, middle and end of the month are 23h. 47m., 22h. 56m., and 22h. respectively. The planet is stationary on Feb. 11. Saturn can be easily identified as it is a little south of Mars and can be observed throughout most of the night.

Announcements

DR. S. WHITEHEAD has been appointed director of the British Electrical and Allied Industries Research Association, of which he has been acting director since 1944.

MR. J. BELL has been appointed chief research engineer of Messrs. Muirhead and Co., Ltd., as from March 1. Mr. Bell, who has been chief of the electrical group at the Admiralty Gunnery Establishment, has been associated with many developments, including remote indication and transmission units (magslips); also servos, electrical computing mechanisms and ship stabilization. Mr. Bell was educated at Armstrong College, University of Durham, and received some of his practical experience in shipyards on the Tyne and also at Messrs. C. A. Parsons.

THE 1946 Conference of the X-ray Analysis Group of the Institute of Physics will be held at the Royal Institution, London, during July 9–11. It is hoped that several distinguished foreign men of science will participate in the proceedings. Further information will shortly be available from the honorary secretary, Mr. F. A. Bannister, Department of Mineralogy, British Museum (Natural History), Cromwell Road, S.W.7.

LETTERS TO THE EDITORS

The Editors do not hold themselves responsible for opinions expressed by their correspondents. No notice is taken of anonymous communications.

Friedel – Crafts Catalysts and Polymerization

DURING the past four years we have been investigating the mechanism of two closely allied reactions—the polymerization of isobutene and the dimerization of di-isobutene. Both these reactions are catalysed by Friedel – Crafts catalysts. Although we have not yet brought these investigations to the point where the mechanism can be definitely established, we have accumulated sufficient data to draw the following conclusions. In order that the reaction may take place at an appreciable rate, it is not sufficient that only the Friedel – Crafts catalyst and the monomer should be present together in the same system; the presence of a third component, probably water, is necessary for rapid reaction to occur.

1. Hexane was shaken in an atmosphere of boron trifluoride in a high-vacuum apparatus and then introduced directly into di-isobutene contained in a closed Dewar vessel in an atmosphere of dry nitrogen. In some of these experiments it was observed that on the addition of the boron trifluoride – hexane solution only a very slow reaction occurred (as measured by the temperature rise). After about 30 min., the reaction had practically stopped, although only about 25 per cent of the monomer had reacted. When, at this point, the Dewar vessel was opened up to the air and the contents stirred gently, the temperature rose rapidly and the reaction proceeded to completion within 2–3 min. This sudden occurrence of the reaction could not be due to the action of stirring, since the solution had been vigorously stirred before opening it to the air.

2. We have also made up the catalyst solution by bubbling boron trifluoride through hexane contained in a burette. Various quantities of this boron trifluoride – hexane solution were added to di-isobutene contained in a closed Dewar vessel. It was found that when a small quantity of the catalyst solution was used, the extent of the reaction depended on the amount of catalyst solution added. Experiments were carried out in which the quantity of catalyst solution added was such as to cause only about 70–80 per cent of the monomer to react. When this limited reaction had stopped, a boron trifluoride – nitrogen stream was bubbled through the solution still contained in the closed Dewar vessel. The partial pressure of boron trifluoride in this gas stream was made the same as that which the catalyst-monomer solution would possess at the initial moment of mixing. It was found in some experiments that this boron trifluoride – nitrogen stream could be bubbled through the solution in the Dewar vessel without further reaction occurring, although as much as 20–30 per cent of the original monomer had not reacted. The boron trifluoride was then blown out of the system by a stream of nitrogen, the Dewar vessel opened to air and two samples of the liquid removed. One of these was distilled and the presence of about 20–30 per cent of unreacted monomer confirmed. The other sample was placed in another Dewar vessel and the same boron trifluoride – nitrogen stream as previously used was bubbled through it. This time the remaining monomer reacted quite rapidly.

3. The reaction of di-isobutene was also studied by bubbling boron trifluoride through cyclo-hexane in a Dewar vessel and adding di-isobutene to the saturated solution. The bubbling of boron trifluoride was maintained throughout the experiment. The reaction followed a first-order law, the asymptote of which, however, often corresponded to only 80 per cent of the monomer having reacted. Further experiments were made in which successive samples of di-isobutene were added to the boron trifluoride-cyclohexane solution, the Dewar vessel remaining closed throughout this sequence of additions, and the bubbling of boron trifluoride being steadily continued. It was found that the first-order rate constant of the di-isobutene reaction varied from one sample to another by as much as a factor of 2.

4. A solution of titanium tetrachloride in hexane was added to a solution of isobutene in hexane in a closed system at a temperature of − 90° C. It was found that a very slow reaction occurred, the rate of which rapidly decreased with time. If laboratory air was blown through the solution even 150 min. after the initial mixing of catalyst and monomer, a rapid reaction took place. This, however, did not necessarily lead to the complete reaction of the monomer (compare section 3), but reaction could be made complete by repeating the blowing through of air. Laboratory air, nitrogen and oxygen, each dried by passage through two liquid-air traps, were not effective in causing the fast reaction. Dried nitrogen re-humified to contain water vapour at a partial pressure of less than 4 mm. was effective.

These results, which have accumulated over a long period of investigation, lead us to the following conclusions: (*a*) In the dimerization of di-isobutene and the polymerization of isobutene, it is essential that a trace of some third component, X (probably water), shall be present in order that the reaction shall proceed at an appreciable rate. The velocity constant of these reactions is not solely determined by the concentration of catalyst and monomer. (*b*) The effect of this third component in accelerating the reaction-rate may cease before all the monomer has reacted. This suggests that the third body X is involved in the initiation of a reaction chain which may be terminated in some way before all the monomer has reacted, and that more X is then required to cause the further reaction of the monomer. Having participated in the initiation of a reaction chain, the third component X is no longer able to initiate further chains. This would be the case if the reaction chain were an energy chain in which the energy of initiation came from the heat of combination of a molecule of catalyst with a molecule of X. It would also be the case if the reaction chain were a proton chain in which the initiating proton was liberated from the third component X when this molecule combines with a molecule of catalyst.

We have not yet mastered the technique of reliably excluding the factor X in the way described in experiments 1 and 2, but this has been achieved by the process described in experiment 4.

We thank Imperial Chemical Industries, Ltd., Billingham Division, for their stimulus, information and supply of samples, which have been invaluable throughout this investigation.

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Identification of Aminobenzoic Acids in Relation to Bacterial Metabolism

THE problem of the chemical identification of *p*-aminobenzoic acid has become important in view of its well-known action as antagonist of sulphonamides and the theory of Fildes and Woods concerning the mode of action of sulphonamides. Microbiological methods will always leave some doubt as to specificity, and the frequently used diazotization methods (suggested, for example, for estimation of *p*-aminobenzoic acid by Eckert[1]) are known to be unspecific and will be shown to give misleading results. So far, the presence of *p*-aminobenzoic acid has been established only for yeast[2,3] by methods requiring large amounts of material.

One of us (R. L.) has worked out a method allowing the identification of *p*-aminobenzoic acid and other arylamines with a far greater degree of specificity. The diazotized compounds are coupled with dimethyl-α-naphthylamine in acetate buffer and the azo-dyes extracted with ether. The dyes derived from aromatic amino-acids are extracted from ether by dilute sodium carbonate, while non-acidic dyes and excess amine remain in the ether. The acidic dyes are brought back into fresh ether by gradual acidification with phosphoric acid and 'chromatographed' on alumina (Merck – Brockmann for chromatographic analysis). The dyes are adsorbed in the top layer. Development with ether containing 2 per cent acetic acid washes the *p*-aminobenzoic acid dye down the column as a sharp orange-yellow ring. Presence of anthranilic acid is at once indicated by the formation of a deep purple ring which moves down the column much more slowly and is eluted by ether containing 20 per cent acetic acid. *M*-aminobenzoic acid is not readily separated from *p*-aminobenzoic acid by this method. The amount of the dyes is estimated by extracting from ether with 20 per cent aqueous hydrochloric acid, and spectrophotometric estimation of the red-violet solution at a wave-length of 530 mμ.

The dye derived from *p-aminobenzoic acid* is identified by the formation of its typical hydrochloride, which on shaking the ether solutions with 10 per cent hydrochloric acid is taken up by the acid, from which it soon crystallizes in fine blue-violet needles with strong green surface colour. It shows no pleochroism under 'Polaroid'. The neutral orange dye, recrystallized from petrol ether, melts at 225° C.

The neutral dye of *anthranilic acid* crystallizes from dilute acetic acid (blue-violet solution) in typical blue-violet needles with strong pleochroism; m.p. 150° C.

The hydrochloride of the *m-aminobenzoic acid dye* crystallizes much more slowly than that of the *p*-aminobenzoic acid dye, in red-violet flat needles with distinct pleochroism. The neutral orange dye melts at 195° C.

With these methods the presence of *p*-aminobenzoic acid in bakers' and brewers' yeast and in normal rats' urine (accompanying other arylamines) was demonstrated. Peptone did not yield *p*-aminobenzoic acid.

An important proof for the theory of Woods and Fildes would be the demonstration that more *p*-aminobenzoic acid is formed by bacteria made resistant to sulphonamides than by the susceptible parent strain. This has been claimed[4] for *Staphylococcus aureus*, *Gonococcus*[5] and *Pneumococcus*[6] on the basis of microbiological methods and diazotization experiments. For *B. coli*, Landy *et al.* were not able to observe a greater *p*-aminobenzoic acid content of the resistant strain.

We have studied a variant of *B. coli communis*. The organism in a few days adapted itself to grow from small inocula in the inorganic medium containing 0·2 per cent glucose. Two parallel series of transfers were begun: (*a*) in the medium alone; (*b*) in the medium containing sulphathiazole. After fifty daily transfers, for both (*a*) and (*b*), the culture fluid of (*a*) did not contain any arylamine before the fourth day of growth, but later about 2 mgm. per l. This arylamine consisted mainly of anthranilic acid and a similar acidic arylamine yielding a red azo dye and a second purple ring in the chromatogram (perhaps derived from kynurenine (β (*o*-aminobenzoyl)-alanine)). Little, if any, *p*-aminobenzoic acid was found. *B. coli* can thus produce anthranilic acid from ammonia as source of nitrogen, either as intermediate in the synthesis of tryptophane or as oxidation product of tryptophane. This shows that the diazotization method for the estimation of *p*-aminobenzoic acid can give erroneous results even in a synthetic medium. The sulphonamide resistant strain (*b*) was transplanted into sulphonamide-free medium, and the culture fluid contained arylamines after 24 hours growth, compared with four days in (*a*). This again consisted mainly of anthranilic acid. No increase of *p*-aminobenzoic acid as compared with (*a*) was found. Therefore, contact with the drug appears to hasten the formation or excretion into the medium or anthranilic acid. Sevag and Green[7] have recently observed effects of sulphonamide resistance on the formation of arylamines from tryptophane by *Staphylococcus* and have assumed this arylamine to be anthranilic acid or kynurenine.

We have lately, however, observed a greater production of *p*-aminobenzoic acid by the organism made resistant to higher concentrations of sulphathiazole. The question is therefore still open and the problem will be investigated further.

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Liebermann–Burchard Reaction for Steroids

THE Liebermann–Burchard reaction for steroids[1], which consists in adding a few drops of acetic anhydride and concentrated sulphuric acid to the chloroform solution of the sterol, is not only used as a qualitative test for it but also for its quantitative estimation in blood, etc. Formation of coloured halochromic sulphate of ketone was suggested by Wieland and Weil[2] in explaining this reaction. The reaction product, when poured into water, was found to be soluble in the latter, and the grass-green coloration was observed. By studying this water-soluble colouring product, it has been possible for us to state definitely that it is the sulphonic acid derivative of cholesterilene, which has previously been reported to

be formed through the action of sulphuric acid on cholesterol dissolved in glacial acetic acid[3]. The acid gives insoluble calcium and barium salts.

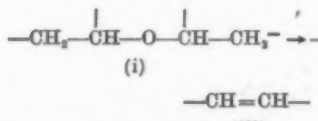
Experimental. 0·5 gm. of cholesterol was dissolved in 2 c.c. of chloroform and 2 c.c. of acetic anhydride were added to it. Next 0·5 c.c. of concentrated sulphuric acid was added drop by drop, the temperature of the reaction vessel being maintained at about 30° C. The colour of the solution was found to change from violet and blue to grass-green. The reaction mixture was allowed to remain for half an hour with frequent shaking. Chloroform was then evaporated off at ordinary temperature and excess of dry ether was added to the acetic anhydride solution. The greenish pigment which was precipitated was filtered and washed repeatedly with ether. In this way excess cholesterol as well as the last trace of sulphuric acid was removed. The precipitate when dried was found to weigh about 0·25 gm. The aqueous solution was found to be acidic in nature and to contain sulphur.

Formation of barium salt of the pigment. The pigment was dissolved in water, and 0·1 *N* barium acetate solution was added to it until no further precipitation occurred. The precipitate was collected on a suction filter, and washed with water. This barium salt on analysis by the usual method was found to contain 13·14 per cent of barium.

Isolation of the pigment could also be effected by means of a second method. The solution of the pigment was allowed to stand for three days. It was then made alkaline with sodium carbonate and allowed to dialyse until the solution was freed from sulphate. The solution was then concentrated to half its original volume; absolute alcohol was added, and the precipitate formed was filtered and dissolved in the least amount of water. This was acidified with dilute hydrochloric acid and then allowed to dialyse, until the solution was freed from chloride. The solution was next concentrated and 0·1 *N* barium acetate solution added to it until no further precipitate was formed. The barium salt thus prepared was collected on a suction filter and washed several times with water. This salt on analysis was found to contain 13·2 per cent barium; $(C_{27}H_{43}O_2S)_2Ba$ requires 13·3 per cent of barium.

Both methods were used successfully to isolate the barium compound of the cholesterilene sulphonate in sufficient yield.

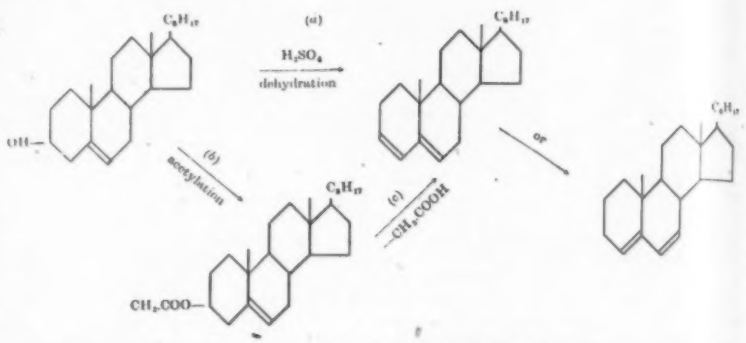
Nature of transformation of cholesterol to cholesterilene. Bills and McDonald[4] assumed the formation of dicholesteryl ether (i) from cholesterol by the action of floridin in acetic acid solution. This dicholesteryl ether was supposed to be further ruptured to cholesterol (ii) and cholesterilene (iii) by Yoder[5], as follows:



But we have found that, apart from the cholesterilene formed in the reaction with sulphuric acid which is insoluble in acetic acid, there is another compound present in the acetic acid filtrate, which melted at 114° C. and gave cholesterol on saponification with alcoholic potash[6].

No intermediate compound as dicholesteryl ether or any free cholesterol could be obtained in the resultant mixture, which indicates a different course of reaction altogether.

This may be represented by either (*a*) or (*b*) and (*c*) continued as follows:



Of these (*b*) and (*c*) seem to be more probable, for formation of cholesterilene has already been observed from the acetyl derivative of cholesterol by means of sulphuric acid[6].

Formation of a violet ring in the Nath reaction[7] with cholesterol acetate is also in favour of this. No red or violet colour would be possible had there been no removal of acetic acid and insertion of the double bond at $C^{4,5}$.

It is interesting to record that a grass-green water soluble pigment was isolated by Nath and Debnath[8] while studying the reaction products formed in the Nath reaction. It was desirable to see if that pigment was identical with the product obtained in the Liebermann and Burchard reaction; and a similar barium compound has been obtained. Yoder[5] also reported the isolation of this pigment in the form of the barium salt with cholesterilene as the starting material. This proves nicely that the cholesterilene, a hydrocarbon, is the intermediate product in the formation of water-soluble sulphonate in the Liebermann–Burchard as well as in the Nath reaction.

Our thanks are due to Prof. J. K. Chowdhury and Dr. J. C. Bardhan for their kind interest in this work.

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Detonation of Liquid Explosives by Impact

ALTHOUGH a considerable amount of experimental work has been done on the impact sensitivity of liquid explosives, the mechanism of initiation by mechanical shock is not fully understood. Furthermore, the values for sensitivity obtained by different workers showed marked variations. With nitroglycerine, for example, the figures quoted range from 14,000 gm. cm.[1,2,3] down to 500 gm. cm. (sensitivity given as the product of mass of striker in gm. multiplied by height of fall in cm.). An attempt has been made to investigate in more detail the physical processes which occur during impact, and to study the mechanism of the initiation and growth of explosion[4,5].

One observation which is of general interest is the profound effect which minute bubbles of included gas have on the impact sensitivity of the explosives[6]. It is found that, under many conditions of impact, minute bubbles are present or are entrapped in the explosive. If one of the surfaces contains a small pinhole or cavity, inclusion of air almost invariably occurs during impact, and even if smooth flat surfaces are used, air is entrapped if the distribution of the explosive is not uniform. If a small amount of gas is present, the explosive may be detonated by the gentlest of blows. With nitroglycerine, for example, when a bubble as small as 5×10^{-3} cm. in radius is present, an explosion may occur with the impact of 40 gm. falling 0·5 cm. (impact energy 20 gm. cm., rate of approach of surfaces 30 cm./sec.). In the absence of gas bubbles, impact energies of the order of 10^{5}–10^{6} gm. cm. and high rates of approach are necessary. The initiation is due to the adiabatic compression and heating of the entrapped bubble. Although the limiting sensitivity varies with different explosives, the effect is quite general and applies to all the liquid, gelatinous and plastic explosives which have so far been studied. It also applies to solid explosives if they are melted or are dissolved in various organic solvents such as benzene and dioxan.

There is some evidence that explosion can take place when the pressure ratio is about 20 : 1, and approximate calculations suggest that the temperature rise in the gas bubble must be *c.* 400° C. for initiation to occur, but it is usually considerably greater. If a gas of low γ, such as ether ($\gamma = 1{\cdot}08$) or carbon tetrachloride ($\gamma = 1{\cdot}13$) is used instead of air ($\gamma = 1{\cdot}4$), the sensitivity is correspondingly decreased.

It is probable that initiation occurs in the vapour phase inside the bubble. The explosion spreads outwards from this tiny hot bubble and is propagated through the surrounding explosive. Photographic and electric measurements show that initiation occurs at, or a few microseconds after, the first moment of impact, and the explosion, which begins at the bubble as a comparatively gentle burning, is rapidly transformed into a more violent explosion with a velocity exceeding 1,000 metres a second. Experiments at low pressure show that the amount of included gas necessary to render the explosive sensitive to these small impacts is minute. The mass of entrapped gas can be as small as 3×10^{-11} gm., and the quantity of heat developed within the bubble during its compression only *c.* 10^{-7} calories. Bubbles of this size may easily be entrapped under many conditions of laboratory and practical operation. Since they are difficult to see even with a microscope, they readily escape detection and, under appropriate conditions, they may constitute a serious hazard.

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Carriage of Plague by the Common Brown Rat (*Rattus norvegicus*)

IN *Nature* of December 8 it is asserted, without comment, that the common rat of Britain (*Rattus norvegicus*) does not carry plague[1].

This belief often arises because plague in the Middle Ages was, at least in Western Europe, carried by the 'black' rat, *R. rattus*; at that time *R. norvegicus* had not spread so far[2]. The spread of *R. norvegicus*, and the decline of *R. rattus* since the beginning of the eighteenth century, have taken place in a period during which there has been relatively little plague in Europe.

The absence of plague is, however, not attributable simply to the absence of *R. rattus*. *R. norvegicus* is well known to be a vector of plague[3]. A clear instance of the relationship between infection of this species and human infection is provided by the recent outbreak in Malta.

The outbreak began in June 1945. The dominant rat in Malta is *R. norvegicus*, which is of general occurrence both in towns and countryside, and even in the port and dockyard areas. During the period October 1944 to September 1945, of the rats examined in the Government laboratories, 36,376 were *R. norvegicus* and 2,903 were *R. rattus*. Up to December 1, 1945, 17 rats had been shown to be infected with plague. Of these, 12 were *R. norvegicus*. One of the foci of infection was the small inland village of Zurrieq, where there were 14 human cases during the period June 23–August 7, 1945. Here *R. norvegicus* was clearly implicated: the first four infected rats were caught in this area, and were all of this species. Three of them were taken from a refuse heap, and Dr. J. Morana, the medical officer of health, clearly established the relationship between this refuse collection and infection of human beings.

The figures of rats examined, and of rats shown to be infected with plague, have been kindly supplied by Prof. V. J. Mifsud, analyst and bacteriologist to the Government of Malta.

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A New Anti-malarial Drug

THE valuable work of I. M. Tonkin and T. S. Work on two new anti-malarial drugs[1] has brought to light the action of a well-known Eastern remedy. *Dichroa febrifuga*, of the family Saxifragaceæ, is a shrub found from China and north-eastern India to Java and the Philippine Islands. In the form of a decoction, the leaves and stems have been used in Indo-China for quotidian, tertian and quartan ague. When in India more than fifty years ago, a native physician sent to me some stems of the plant as a fever remedy and suggested that it might contain an alkaloid similar to quinine. The drug yielded no alkaloid but a glucoside, and differing from other astringent plants of this family it afforded no tannin. Later, a sample of the same drug was sent as a fever medicine used by the Chinese in Malaya. It is satisfactory to note that recent pharmacological research confirms the suitability of the specific name—*febrifuga*—given to the plant by Loureiro (*Fl. Cochinale*) so long ago as 1790.

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[1] *Nature*, **156**, 630 (1945).

Tritanopia and Colour Vision

THE existence of foveal tritanopia, in spite of the convincing observations of d'Alrutz (1909), Richardson–Robinson (1923) and Göthlin (1943), has been denied by those who, like Polack[1], disagree with the trichromatic theory of colour vision and the classical division of dyschromatopsies. But the fact, already pointed out fifty years ago by König, has now been clearly evidenced by the results of Willmer and Wright[2].

Does the cause of that dischromatism lie in the absence of cones specifically ruling the sensation of blue? In my opinion it rather lies in the replacement, in the 'blue' cones, of their specific photosensitive substance by the substance specific of the sensation of green: it is assumed that the common photosensitive substratum of those two receptors causes equal effects, expressed in the form of a 'blue-green' sensation complementary to the 'red' sensation. This hypothesis accounts for the neutral spectral point, where is achieved that equality of action between the 'blue' and the 'green' cone on one hand and the 'red' cone on the other.

In deuteranopia, the photosensitive substance specific for the sensation of red must in the same way be found in the 'red' and the 'green' cones alike, which explains the sensation of yellow brought to light by the monocular anomalies.

It is, however, surprising that the visibility curve is but very slightly altered in deuteranopia as well as in tritanopia, for though the contribution of the blue component is almost negligible, that of the green component is essential.

In this respect, a long series of facts have led me to dissociate entirely light vision and colour vision[3]. I consider that the building up of the spectral response curves of the fundamental colours from chromatic equations, and the use of visibility coefficients very different from the coefficients of chromatic efficiency in order to fall back on the general curve of foveal visibility, are both based on an assumption which, in my opinion, is groundless. (For the same chromatic valence the quantities of light contributed by the 650, 530 and 460 mμ waves are, respectively (according to Wright), 0·648; 1·0; 0·05.)

I admit that the receptor unit is made of a tetrad of cones and that three of those contain a photosensitive substance having, for each of them, different constants of absorption, with antagonistic interactions annulling the response when the three systems are equally stimulated; the fourth includes the three substances at the same time, but in unequal proportions (which accounts for the unequal efficiency of monochromatic waves), and brings on the sensation of light, that is, a sensation chromatically neutral.

If the 'green' cones include the substance for the sensation of red, and if in addition the neutral cone includes the 'green' substance, the visibility is not modified.

However, we can admit a slight contribution of the specific chromatic cones to the ganglion cell that gives the light message. When the 'green' or the 'blue' substances are missing in the corresponding cones, it is admitted that that chromatic modification accounts for the slight alteration of the visibility curve in deuteranopia and tritanopia.

In protanopes, the notable spectral shortening may be due to the lack of the 'red' substance, not only in the specific cone but also in the neutral cone which plays an essential part in the sensation of brightness.

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Bud Rot of Areca Palms in Mysore

IN Hirethota, a village near Thirthahalli, Mysore State, a serious disease of areca palms has been rampant for the past fifty years. Locally the disease goes by the name 'hidimundige', which literally means the thinning out or tapering of the crown, gradually dwindling in diameter. But actually the disease in no way resembles the tapering stem wilt, better known as the 'pencil point disease' of coco-nuts so well known in the West Indies. A preliminary report of the investigations carried out is presented here.

On casual observation, no marked difference appears to exist between the healthy plants and the diseased ones. But an experienced observer can always spot the diseased plants in which the bud rot has set in, thereby preventing further growth of the plants. The leaves are gradually shed one after the other and finally the entire crown slips out, due to the rot at the base, leaving the bare stem. The time required for the infection to set in, to the stage of the final death of the trees, is spread over a period of several years. The yellowing or the wilting of the pinnæ, so characteristic of the red ring disease and the bronze leaf wilt of coco-nuts, does not appear to be a diagnostic character of the present disease.

The apices of several affected plants were cut and examined at various stages of infection. In the early stages, the discoloration of the still unfolded leaves in the stem apex is noticeable. This discoloration gradually extends downwards into the meristem, and finally the entire central spindle begins to rot, becoming converted into a soft, evil-smelling mass of tissue. The decay is enhanced by the secondary infections of bacteria and *Fusarium* sp. As pointed out by Nowell[1] and Briton-Jones[2], the findings of

Johnston[3] that the bud rot of coco-nuts is caused by *Bacillus coli* is no longer tenable.

Detailed microscopic examination of the affected parts reveals the presence of a species of *Aphelenchus*. In the partly decaying meristematic tissues of the bud and the central core of the wood at various levels, such a large number of nematodes were noticed that it was beyond doubt that they were parasitic. Many of the nema were found encysted within the host tissues as globular bodies and many others were found making their way within the lumen of the vessel members and breaking them into fragments. Longitudinal splitting of the stem indicated the discoloration and the deterioration of the central core all through the stem. It was manifest that the bud rot was due to the breaking down of the vascular strands within the stem. In one of the trees about 40 ft. high, I bored a hole into the stem, about waist-high from the ground, in order to examine the central core of diseased tissue. Immediately, to the surprise of many present there, a spout of evil-smelling fluid gushed out with great force in appreciable quantities (to the extent of a gallon). Microscopic examination of this fluid revealed the presence of thousands of eggs of the nematode with fragments of xylem cells and associated with other saprophytic organisms.

The red ring disease of coco-nuts was first described from the British East Indies by Nowell as due to the parasitic nematode *Aphelenchus cocophilus* Cobb. The distribution of the disease is so far confined to the tropics of the western hemisphere and restricted to the genus *Cocos* alone. The nematode parasite observed in the areca palm by me resembles the parasite described by Cobb[4] in a general way. The following facts should be considered in connexion with the identity of the organism: (*a*) *Aphelenchus cocophilus* Cobb is said to be restricted to *Cocos* alone without having any host range on other palms and is so far unknown in India. (*b*) The infected stems of coco-nut palms develop a red band or ring in the stem, a feature absent in the present case. The occurrence of such a red ring might, however, be symptomatic of the particular host. Until authentic specimens of *Aphelenchus cocophilus* are available for comparison, the identity of the present species in areca palms remains obscure.

Nowell enumerates the various modes of the spread of the red ring disease of coco-nut, and it is possible that even in Mysore some fauna are concerned in the transport of the disease. In some instances, areca nut seedlings taken from Hirethota gardens to other plantations near by have carried with them the infection and established the disease in new places. The infectious nature of the disease has thus become evident and strict measures should be adopted in future in isolating the disease. It has also precluded the older conception that the 'hidimundige' disease of areca palms is due to a 'physiological drought' of the soil.

Grateful thanks are due to Dr. L. N. Rao, professor of botany, for much encouragement.

M. J. THIRUMALACHAR.

Department of Botany,
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[1] Nowell, W., "Diseases of the Crop Plants in the Lower Antilles", West India Committee (London, 1923).
[2] Briton-Jones, H. R., "Diseases of Coconut Palm" (London: Baillière, Tindall and Cox, 1940).
[3] Johnston, J. R., "The Bud Rot of the Coco-nut Palm", U.S. Dept. Agric. Bur. Plant Ind., Bull., 228 (1909).
[4] Cobb, N. A., "Red Ring Disease, a Newly Discovered Nematode (*Aphelenchus cocophilus* n.sp.) connected with a Serious Disease of Coco-nut Palm", *West Indian Bull.*, 17, 203 (1920).

Archæological and Palæontological Exploration in South Africa

IN *Nature* of June 2 (p. 675), there appeared an article entitled "Archæological Exploration in South Africa", in the course of which Mr. M. C. Burkitt expresses fears about the wisdom of controlling unlicensed persons from excavating sealed archæological deposits.

The body responsible for the framing of the laws referred to is the Commission for the Preservation of Natural and Historical Monuments, Relics and Antiques. Among its twelve members are a geologist, a palæontologist, a physical anthropologist and an archæologist. These sister sciences are therefore well represented by responsible scientific men.

The Commission functions under the Natural and Historical Monuments, Relics and Antiques Act of 1934 as amended by the Natural Monuments Amendment Act of 1937. Among the enactments we find one which prohibits:

"(1) the *removal* of any drawing or painting on stone or petroglyph known or commonly believed to have been executed by Bushmen or other aborigines of South Africa or by any people who inhabited or visited South Africa before the advent of the Europeans, and any *in situ* implement or ornament known or commonly believed to have been used by them and any anthropological or archæological contents of the graves, caves, rock shelters, middens or shell-mounds, without the written consent of the Commission; and

"(2) the *removal* of any *in situ* vertebrate fossil without the written consent of the Commission;

"(3) *provided* that this prohibition shall not apply to removals that result from the normal activities of *bona fide* mining, engineering and agricultural enterprises".

This simply means that anyone is free to collect stone implements and vertebrate fossils found lying loosely on the surface, and that the written consent of the Commission is needed for excavations only; the whole object of the Commission being not to restrict archæological and palæontological exploration, but to control it and to encourage methodical work.

Burkitt's statement that the problems which we in South Africa have tackled apply equally to Great Britain is, I submit, open to argument. One cannot fruitfully compare the unusual wealth of the archæological and palæontological fields of the Union with the comparative poverty of these fields in Great Britain.

Burkitt's comments were followed by further criticism in *Nature* of July 14 (p. 55). Prof. D. M. S. Watson approves of the archæological legislation but maintains that "vertebrate fossils are in an entirely different category", and adds the remark that the palæontological legislation "is for the protection of the South African palæontologists, who do not appear to wish their colleagues to examine the new materials". Little could be further from the truth. South African palæontologists are not responsible for the legislation. The responsible body, the Commission for the Preservation of Natural and Historical Monuments, Relics and Antiques, naturally consulted several leading palæontologists, who, with a single exception, approved of the protective measures because they

too wished to see the public educated and more system introduced into excavations. As an educative measure in the first instance, the Commission prohibited the removal of all vertebrate fossils, whether *in situ* or loose-lying, without the written consent of the Commission; but when the very considerable discussion which followed had received the widest local publicity and the Commission was satisfied with the extent of the public appreciation of the issues involved, the prohibition was relaxed and made referable to *in situ* specimens only.

The legislation under discussion is supported by the South African Museums Association, a body which recently recommended an extension designed to prohibit the unlicensed removal of *in situ* invertebrate fossils as well. The Commission therefore has full scientific support despite the fact that it has not gone quite so far as such a representative, responsible and expert body as the Museums Association would have it go.

In both the archæological and the palæontological fields the restrictions are imposed solely to preserve such evidence as only *in situ* specimens can yield—and no amateur need hesitate to apply for permission to explore these fields. He will find the Commission eager to do everything possible to enable him to carry out his work. All that is required of him is to prove his competence and his earnest.

To dispose of Prof. Watson's assertion that the restrictions are for the protection of South African palæontologists, it need only be remarked that no less than five invitations have recently been extended to overseas palæontologists to visit and conduct field-work and excavations in South Africa—and if more wish to come they have only to prove their competence to be assured of a very warm welcome. In fairness to Prof. Watson, however, it must be recorded that his criticism was based on a law that prohibited the removal of any vertebrate fossils whether *in situ* or loose-lying on the surface—a law that is now obsolete.

C. van Riet Lowe.

Commission for the Preservation
of Natural and Historical Monuments,
Relics and Antiques,
University,
Johannesburg.

I am afraid I am not completely convinced: the very length and warmth of Prof. van Riet Lowe's reply rather increases my uncertainty. As a necessity of war, we have had much experience of bureaucratic controls in Britain—far more than Prof. Lowe can have ever experienced—and there is no doubt but that the result is a stifling of individual effort. Prof. Lowe suggests that the situation in England is not comparable with that in South Africa, as the former country is so poor in archæological remains compared with the sub-continent. Surely that would be an argument for more controls in Britain than there!

Of course, it is always a question of the way an Act is interpreted and administered. So long as Prof. Lowe is in charge all will be well; but his successor may be a quite different sort of individual. Committees move slowly and tend to meet rarely—I have much personal experience as a county councillor—and I fear that permits to excavate will be a long time in arriving. Again, does the bureaucracy intend to set up a system of inspection? South Africa is an individualistic country with a scattered population. Frankly, if I had a rock-shelter on my farm and wished to dig, I should write to Prof. Lowe, as I know I should receive from him immediate and sympathetic treatment. Should his successor be a pure bureaucrat, I should be tempted just to go ahead and no one would be the wiser. But I could not publish my results, and so my information would be lost to science.

M. C. Burkitt.

Since the publication in *Nature* of July 14, 1945, of my letter about the South African restrictions on the collection of vertebrate fossils, I have received letters from two South African palæontologists, one of whom supports, while the other opposes, the Act in question; but both agree that its intention was to prevent the collection of such materials by South African amateurs. I am extremely glad to learn that there is no intention of interfering with the activities of visiting men of science.

The restriction was based on the idea that an amateur collector might remove the more obvious part of a specimen, leaving behind other parts of importance, and thus destroy valuable evidence. But this real danger must be viewed in the light of the fact that there are four vertebrate palæontologists in South Africa, three of whom on account of their age or official position are unlikely to do much collecting in the future, and that the great majority of the really new animals found during the past twenty years have been collected by amateurs, some of whom do excellent work.

As originally proclaimed on September 20, 1938, a fine of £25 was imposed on anyone who, without the written consent of the "Museums" Commission, removed a fossil bone. If enforced, this regulation would merely have ensured that very many fossils would have followed a 15-ft. skeleton which, in a few years before 1929, was washed away at Lady Frere. On June 23, 1945, when no prosecution under the original order had been attempted, it was modified to allow of the collection by anyone of fossils lying on the surface, though not their removal from the rock. The effect of this new regulation would be to compel amateurs to leave in the rock the buried parts of a skeleton of which the head had fallen loose: in fact, to encourage the kind of damage the original order was designed to prevent.

It is, I think, clear that neither order could possibly be enforced against a South African, but it is evident that in either form it could be employed against a visitor to that country, hence my original mistaken interpretation!

In any event the order comes unfortunately from a country which has for some twenty years tolerated the existence in a national museum of a great collection of fossil vertebrates containing many important specimens, still undescribed and in practice inaccessible to palæontologists.

The letter from Prof. van Riet Lowe which precedes this is perhaps a little disingenuous, because it mentions few dates and does not make it clear that the five American and German collecting expeditions to which he refers preceded the National Monuments Amendments Act of 1937, a matter that was in my mind at the time of my original letter.

D. M. S. Watson.

NICOTINIC ACID IN SUGARY AND STARCHY MAIZE

By Dr. K. MATHER
John Innes Horticultural Institution
AND
Dr. E. C. BARTON-WRIGHT
Cereals Research Station (Ministry of Food), St. Albans

IN 1944 Barton-Wright[1] reported the nicotinic acid contents (expressed in μgm./gm.) of the grain of a number of cereals, including eleven strains (varieties and hybrids) of maize. Four of the eleven were sweet corns grown in Britain, and the other seven flint corns of various origins. It appeared from his figures that the two groups differed in nicotinic acid content, the sweet corn having on the average twice the content of flint corn (31·0 μgm./gm. against 15·6 μgm./gm.).

A further test was arranged in the summer of 1944 on a wider range of strains grown together at Merton, to check and extend this finding. Twenty-four sugary and five starchy strains were grown in single-row plots in the same piece of ground. The sugary strains were all, of course, sweet corns; but the five starchies comprised four flints and one dent. Three of the flints and two sweets were among the strains used in the previous tests. Owing to unforeseen circumstances it proved impossible to make the controlled pollinations which had been planned. Ears were therefore harvested from the twenty-nine strains after natural open pollination. Though this must detract from the value of the data, we feel that the results are of sufficient interest to warrant recording, more especially as it is our experience that in the circumstances of the experiment most of the open pollination would be with other plants of the same strain.

The nicotinic acid content of the maize samples was determined microbiologically by the method described by one of us[1] with only minor modifications, for example, the addition of 0·3 per cent ammonium sulphate to the medium and the application of the Carpenter and Strong[2] extraction procedure to the samples. The values of each assay were calculated by the slope-ratio method.

Five of the sweet corns gave ears which, owing to the open pollination, bore some starchy grains among the sugary ones. The fertilizing pollen grains in these cases must have been from starchy plants. The two kinds of grain were assayed separately, with the results shown in Table 1.

TABLE 1.

Mother strain	Nicotinic acid content (μgm./gm.) Sugary grain	Starchy grain
Country Gentleman	33·3	15·1
Early Yellow Sensation	23·7	16·1
Fogwill's Early	23·7	13·7
$M_{13} \times M_{14}$	28·3	12·4
$M_6 \times$ Wisconsin 212B	22·2	12·7
Mean	26·2	14·0

The chance probability of a difference as large or larger lies between 0·01 and 0·001, and so it must be deemed significant. The ratio of nicotinic acid contents is, as before, about 2 : 1.

The remaining nineteen sweet corns gave an average of 29·5 ± 1·54 μgm./gm., with a standard deviation of 6·72 and extreme values of 43·5 and 21·4. The mean of the five starchies was 15·8 ± 1·27 μgm./gm., the standard deviation 2·83 and the extremes 18·2 and 11·2 μgm./gm. The difference between these means is again significant ($P < 0·001$) and the ratio of the means is nearly 2 : 1. The dent corn gave 15·0 μgm./gm., and so was indistinguishable from the four flints.

The difference between the contents of sugary and starchy grain cannot be attributed to the greater shrivelling of sugary seeds in drying, for the ratio of the mean weights of neighbouring sugary and starchy seeds from two mixed ears was 1 : 1·1.

Sweet corn characteristically carries the 'sugary' allelomorph (*su*) of a gene which is represented in flint and other starchy corn by the dominant 'starchy' alternative (*Su*). The endosperm (the nicotinic acid content of which we are presumably largely determining) is, of course, triploid, that in sugary seeds being *su su su*, and that in true-breeding starchy seed being *Su Su Su*. In hybrids the endosperm is always phenotypically starchy even though it has the constitution *Su Su su* where the mother was starchy, and *Su su su* in the reciprocal. The starchy grain from sugary mothers (Table 1) was thus *Su su su*. Besides appearing starchy, it had a nicotinic acid content within the range given by seed coming from starchy mothers, and being therefore *Su Su Su* or *Su Su su* in endosperm constitution. The nicotinic acid difference thus follows the action of the *Su — su* gene, even to its dominant relations. The nicotinic acid difference may, therefore, be provisionally ascribed to the *Su — su* difference, though there must remain an element of doubt until grain has been obtained which differs only in this gene, the genetic background being the same in all samples.

In any event, there are indications of the action of other genes. A sample from a Golden Bantam mother plant gave 35·2 μgm./gm. nicotinic acid, while three samples from Golden Cross Bantam mothers, of different origins, gave 36·0, 35·3 and 43·5 μgm./gm. nicotinic acid. (The latter value is very high for maize but was confirmed by an assay of 46·4 μgm./gm. in a second test.) Although varying between themselves the values for all four strains are well above the sweet corn average. Two other mother plants, of John Innes Hybrids 1 and 2, were hybrids with a common parent. They gave 23·4 and 24·5 μgm./gm. nicotinic acid respectively. The hybrids $M_6 \times Wis$ 212*B* and $M_6 \times Wis$ 204*A* also had a common parent and gave values of 22·2 and 25·5 μgm./gm. nicotinic acid. Thus a measure of common ancestry is associated with similar nicotinic acid contents in the three cases where this test could be applied. All these strains were sweet corns and so the effect of related ancestry must be due to genes other than *Su — su*.

Some of the strains had yellow and the others white endosperm. This difference is known to influence the vitamin A content, the amount being proportional to the number of *Y* (yellow) allelomorphs present[3]. Dominance is thus absent in respect of this action, in contrast to that of the *Su — su*

TABLE 2.

	Starchy			Sweet	
Year	Quebec 28	Jehu	Beacon	J. I. Hybrid 1	J. I. Hybrid 2
1943	15·0	20·0	18·0	28·0	33·0 μgm./gm. nicotinic acid
1944	18·2	17·3	11·2	23·4	24·5 μgm./gm. nicotinic acid

gene on nicotinic acid content. The data failed, however, to show any clear indication of the yellow-white difference affecting nicotinic acid content.

An effect of environment is also suggested by the contents of five strains in the two years 1943 and 1944 (see Table 2). In all but Quebec 28, the 1944 content is much below that of 1943. The difference is not, however, significant (P between 0·20 and 0·10) and so the data cannot be regarded as clear on this point.

Finally we may observe that sweet corn approaches English wheat (48·0 μgm./gm.) in nicotinic acid content. This in turn is a little poorer than Manitoba wheat (55·0–66·0 μgm./gm.), and much poorer than barley (91·5–147·0 μgm./gm.). On the other hand, even starchy maize is higher than oats (4·9–7·0 μgm./gm.) and rye (7·7–10·0 μgm./gm.)[1].

[1] Barton-Wright, E. C., *Biochem. J.*, **38**, 314 (1944).
[2] Strong, F. M., and Carpenter, L. E., *Ind. Eng. Chem.*, Anal. Ed., **14**, 909 (1942).
[3] Hayes, H. K., and Immer, F. R., "Methods of Plant Breeding" (McGraw-Hill Co., Ltd.; 1942).

SCIENCE AND THE PUBLIC

"THE Publicity of Science, with particular reference to Chemistry" was considered in relation to the Press, exhibitions, films and broadcasting at a discussion arranged by the London and South Eastern Counties Section, Royal Institute of Chemistry, on October 20.

Dr. O. J. R. Howarth, secretary of the British Association, said that there is an increasing public interest in science, for which neither science nor the Press is as yet making adequate provision. It is probably fair to say that education has not yet caught up with the interests of the people. The languages of the sciences are unfamiliar, and men of science do not always realize this. Among them, gifted exponents to the lay public are uncommon; among journalists, those of scientific training sufficient to fit them as scientific correspondents are as yet not numerous. Press news of science could be improved (with the assent of editors) by qualified writers, either free-lance or as scientific representatives of particular newspapers or agencies. The American Science News Service is a strong and efficient organisation, but it has been asserted, whether justifiably or not, that in Great Britain the field for syndicated news is not wide enough to sustain a separate science news agency. The present time does appear opportune, however, for a practical review of the whole question. How should this be initiated? Probably initiation does not lie with science; but if from the side of the Press there should come a strongly backed request for help in the provision of authoritative scientific news, this ought not to find science unresponsive.

Mr. O. F. Brown, chief information officer of the Department of Scientific and Industrial Research, considered that exhibitions provide the most satisfying form of publicity media. In presenting a scientific exhibition it is essential for the man of science and the display artist to work in full mutual understanding. The exhibits and their component parts must attract attention, hold interest and leave a lasting impression. Stands should be designed to suit exhibits. Dioramas and good photographs are attractive, and lighting devices for fading one object into another have great possibilities for illustrating changes which a substance undergoes. Movement can be introduced effectively into unlikely exhibits. Demonstrations excite interest, but short loops of film projected on to ground-glass screens, and running continuously, may well be used to illustrate laboratory processes.

Exhibitions serve to make new contacts; and it is essential, therefore, that the demonstrator should know his job and possess the right personality. The presentation of an exhibition develops a man's confidence, and as self-confidence is essential to the successful research worker, participation in producing exhibits could be justified for this reason alone.

Mr. George A. Jones, of the Scientific Film Association, emphasized the profound effect which the film, employing the combined effects of motion, colour and sound, can have on the regular cinema-goer; and yet such an individual is ill-informed about science and rarely connects it with the food he eats and the clothes he wears.

The growth of scientific film societies throughout Britain indicates an increasing public demand for information on science. Most of the films available have been designed primarily for training, propaganda or advertising purposes, but fortunately many of the so-called documentary and instructional films have dealt with the relation between science and our daily lives. The instructional films have treated this factually, while the documentaries have been more concerned with interpretation. Many of the latter have exhibited a true scientific outlook. The film-maker must replace the unscientific and pseudo-scientific films by new ones based on a scientific outlook, and produce new films dealing not only with scientific technology but also with the essentials and meaning of science, and its effect upon the modern world. Some films already do this; those in which chemistry is concerned include "D.D.T.", "Enough to Eat", "Malaria", and "Distillation". Interpretative films are also needed to bring home to scientific workers themselves the wider part they play—that they are not only experts on certain technical subjects but also specialists in the logical, detached examination of facts and in making deductions from those facts.

Mr. Vincent Alford, acting assistant director of talks, British Broadcasting Corporation, described the unique advantages of broadcasting as a publicity medium. In addition to reports of scientific progress in the news, science is projected into the radio programme by 'features' and talks. Feature technique calls for the co-operation of scientific workers with producers and the employment of script writers to dramatize aspects of science, actors to impersonate scientific men, and the ancillary aspects of music and effects to heighten the dramatic impression; but there must be no sacrifice of accuracy to dramatic effect. By contrast, a broadcast talk is the unadorned spoken word. Speakers should therefore be selected for their ability to take infinite pains to interpret their subject in a language that at once rouses interest and remains intelligible to the audience. Features and talks are complementary, for while the former have a wider audience, the latter are better for explaining points. Experience shows that it is easier to ensure good broadcasts on physics or biology than on chemistry. Chemistry presupposes the knowledge of a basic vocabulary which is not learned in the elementary school and which cannot be taken for granted. Science broadcasts for schools have been helping to build up a science-conscious generation to follow ours. It may be considered that the ration of

science in the talks programmes of the B.B.C. is not enough, but in the congested home service schedules, politics, sociology and straight service talks have to compete in a quota above which the listener will not devote attention. At present, "Science Magazine", on the home service every fortnight, and "Science Notebook", in the short-wave overseas service, merit consideration.

Contributions to the discussion came from speakers directly concerned with the scientific and technical Press, films and broadcasting. Among the points made were that scientific men are partly to blame for unsatisfactory reports on scientific matters in the Press because of their dislike of publicity and their attitude to those who try to interpret such matters to the layman. Hence those who can write well should do so. The censorship of scientific news creates a vacuum which tends to fill with unreliable information; rather should the scientific man give an interview and take the reporter into his confidence, but refrain from extensive statements by telephone. The 'pipe-line' connecting the scientific world and the Press is too narrow, scientific information has to pass through several stages of digestion before it is published in the lay Press, and newspaper editors do not appear to realize the existence of the great public interest in scientific matters. The Press should have scientific editors, who should have a wide knowledge of the sciences and adequate contacts to enable them to check scientific news; they should be able to rely on the professional and learned societies for aid. A 1951 Exhibition, already mooted, should be supported by the professional institutes of the sciences. Historical and biographical films of sciences and scientific men should be encouraged; the Ministry of Education and the learned and professional scientific bodies could assist in these productions. The success of "Science Magazine" depends upon the scientific men who write the scripts; only one idea at a time should be transmitted, in a concise manner.

It was suggested by several speakers that groups in local sections of professional bodies, or an informal society or group for people interested in the publicity of science, should be formed to further, by exchange of ideas and experiences, the presentation of science to the public.

ANTHROPOLOGY IN PORTUGAL

THE history of any branch of science is a very intricate affair as it entails national and international claims of priority, and one is at a loss as to whether the contribution of a particular school is of greater or less significance. The problem is still further complicated when the protagonists are still alive. The history of anthropology in Portugal is worthy of notice by itself, if only because Portugal was one of the great colonial Powers whose influence can still be felt in Brazil, Africa, India and the East Indies.

Pereira da Costa, Carlos Ribeiro and Nery Delgado founded a Geological Society in Lisbon in 1857. In 1865 Pereira da Costa published his "Notices on Human Skeletons discovered in a Cave by Arruda". In 1885, a Ministerial decree authorized the reorganization of museums, and the director asked Barros da Cunha to examine some Timor skulls deposited there. The museum was divided into sections of botany, zoology, geology and mineralogy, anthropology and archæology, each with its own director. From this time dates the systematic collection of materials from the Portuguese Colonies.

On July 2, 1885, Dom Luiz passed a further decree creating a Department of Anthropology, Human Palæontology and Archæology in the Faculty of Philosophy of the University of Coimbra. Two years previously, Bernard Machado had brought up a Bill creating Departments of Agriculture, Rural Economy, Mines and Zootechnology in the University.

In the earlier phase emphasis was laid on prehistoric archæology, particularly on the existence of tertiary man in Portugal, because of the interest shown in the discovery of da Arruda in the grottoes of Furninha and Cesareda, and the graveyards of the Tejo Valley. In the second phase physical anthropology came to the fore, partly owing to the zeal of Ferraz de Macedo and Arruda Furtado. Macedo began his craniometric studies on the remains found in the old cemeteries of Lisbon in 1882; and in the following years he devoted himself to criminal anthropometry with good results. The methods standardized by him have proved useful to subsequent workers in this field.

The first professor of anthropology at the University of Coimbra was Henrique Texeira Bastos, who divided the subject into: (1) zoological and general anthropology and (2) prehistoric archæology. Section 1 dealt with the osteology and craniometry of primates and physiological differences between them and man. The position of man among the primates was also treated. In osteology, the comparative osteology of man was studied. In this section were included methodology, anthropometry, statistics, etc. Section 2 treated the comparative history of man and human races, cultural anthropology, linguistics and prehistoric archæology. The Faculty of Letters taught ethnology for a semester to those who were graduating in geography, but as a matter of fact the subject received but scant attention.

In 1911, Prof. Barros da Cunha began a free voluntary course on colonial anthropology at the Institute of Anthropology and at the Colonial Museum, the subjects being customs, religion, dress, art, folklore and languages. Soon after, monographs on places of local interest began to be published in the *Transactions* of the Section of Anthropology of the University of Coimbra.

Bastos was succeeded by Bernard Machado, whose enthusiasm and interest in physical anthropology ushered in a new period of intense activity. The series of monographs published in the *Transactions* attest his powers of classification and his unusual activities in all branches of science. But Machedo's work could not properly come under orthodox anthropology; for one thing, his treatment was too diffuse; and for another, he was interested in too many things at the same time.

Two other names stand out in Portuguese anthropology, namely, Anthony da Costa Ferreira and Ferraz Macedo. The latter may be claimed as the true founder of physical anthropology in Portugal, for his craniometric investigations provided a basis for later workers.

With the reform of the University of Coimbra in 1911, two institutes of anthropology were established, one at Oporto and the other at Lisbon. At the same time, at Coimbra the curriculum was reorganized and anthropology was divided into physical and cultural. The influence of social and climatic factors was actively studied and attention was directed to palæontology; and as an ultimate

objective, the integration of anthropological and genetic data was envisaged as a general science under natural science.

Genealogical and statistical methods were devised to help craniology and sociology. Since 1907 the Institute has acquired more than 5,000 volumes and 100,000 index cards of measurements.

The Institute at Coimbra has, under Prof. Eusebio Tamagnini, turned out a group of young and active workers in the field of physical anthropology. The names of J. Serra, A. Lopes, A. Themido, W. L. Stevens and de Morais are familiar to workers in Europe and America. The output of original work produced during the War gives promise of greater things to come.

R. E. G. Armattoe.

FORTHCOMING EVENTS

Monday, January 28

Royal Society of Arts (at John Adam Street, Adelphi, London, W.C.2), at 1.45 p.m.—Dr. S. G. Hooker: "High Speed Flight" (Cantor Lectures, 2).

Institution of Electrical Engineers (at Savoy Place, Victoria Embankment, London, W.C.2), at 5.30 p.m.—Discussion on "Electrical Aids to Coal Production" (to be opened by Mr. R. Crawford).

Royal Geographical Society (at Kensington Gore, London, S.W.7), at 5.30 p.m.—Mr. F. R. Newens: "In Northern Waters" (kodachrome films of Iceland, Norway and Denmark, with Commentary).

Chemical Society (joint meeting with Swansea University College Chemical Society, in the Chemistry Lecture Theatre, University College, Swansea), at 6 p.m.—Prof. J. Kendall, F.R.S.: "The Separation of Isotopes and Thermal Diffusion".

Association of Austrian Engineers, Chemists and Scientific Workers in Great Britain (at the Austrian Centre, 69 Greencroft Gardens, London, N.W.6), at 7.15 p.m.—Dr. W. Saraga: "Electric Networks, some Aspects of General Interest".

Tuesday, January 29

Royal Statistical Society (at the London School of Hygiene and Tropical Medicine, Keppel Street, London, W.C.1), at 5.15 p.m.—Symposium on "Serial Correlations".

Royal Anthropological Institute (at 21 Bedford Square, London, W.C.1), at 5.30 p.m.—Dr. Oliver Garrod: "An Account of the Kashgai Tribe of Fars".

British Institution of Radio Engineers, London Section (at the Institution of Structural Engineers, 11 Upper Belgrave Street, London, S.W.1), at 6.15 p.m.—Mr. R. Spears: "Gold Film Electrodes for High Frequency Quartz Plates".

British Institution of Radio Engineers, North-Western Section (in the Reynolds Hall, College of Technology, Sackville Street, Manchester), at 6.45 p.m.—Flight-Lieut. C. Bovill: "Aircraft Radio".

Institution of Electrical Engineers, London Students' Section (at Savoy Place, Victoria Embankment, London, W.C.2), at 7 p.m.—Problems Night.

Wednesday, January 30

Royal Society of Arts (at John Adam Street, Adelphi, London, W.C.2), at 1.45 p.m.—Mr. Kenneth Holmes: "The Place of the Art School in the Life of the Community".

Physical Society, Colour Group (in the small Physics Lecture Theatre, Imperial College, Imperial Institute Road, London, S.W.7), at 3.30 p.m.—Mr. R. Donaldson: "A Colorimeter with Six Matching Colours"; Mr. T. B. Davenport: "A Direct-reading Photoelectric Spectrophotometer".

Institution of Electrical Engineers, Radio Section (at Savoy Place, Victoria Embankment, London, W.C.2), at 5.30 p.m.—Mr. D. I. Lawson, Mr. A. V. Lord and Mr. S. R. Kharbanda: "A Method of Transmitting Sound on the Vision-Carrier of a Television System".

Institute of Welding (at the Institution of Civil Engineers, Great George Street, London, S.W.1), at 6 p.m.—Capt. R. B. Croft and Capt. O. Lithgow: "Mobile Welding with the Royal Engineers".

Thursday, January 31

Queen Mary College (in the Hatton Lecture Theatre, Queen Mary College, Mile End Road, London, E.1), at 4.30 p.m.—Prof. Edmund Giffen: "Engineering Research in the University" (Inaugural Lecture).

Association for Scientific Photography (at the Caxton Hall, Westminster, London, S.W.1), at 6.30 p.m.—Mr. Henry T. F. Rhodes: "Forensic Photography".

Friday, February 1

Society for Visiting Scientists (at 5 Old Burlington Street, London, W.1), at 7.30 p.m.—Discussion on "Science in Czechoslovakia" (Speakers: Prof. J. Bělehrádek, Dr. J. Cisař, Sir Alfred Egerton, F.R.S., Dr. Nora Wooster).

APPOINTMENTS VACANT

Applications are invited for the following appointments on or before the dates mentioned:

Lecturer in Physics—The Principal, Huddersfield Technical College, Huddersfield (January 31).

Teacher of Engineering Subjects—The Principal, Technical School, Oldbury, Worcs. (February 2).

Lecturer or Senior Lecturer (woman) in Mathematics, and a Lecturer or Senior Lecturer (woman) in Gardening, at the Bingley Training College—The Chief Education Officer, County Hall, Wakefield (February 4).

Teacher (full-time) of Chemistry—The Principal, Harris Institute, Preston (February 7).

Resident Engineer (temporary), and Assistant Resident Engineers (2, temporary)—The Water Engineer, Liverpool Corporation, 55 Dale Street, Liverpool 2 (February 9).

Teacher (full-time) of Mathematics—The Principal, Enfield Technical College, Queensway, Enfield, Middx. (February 9).

Assistant Director of Examinations (temporary)—The Secretary, Civil Service Commission, 6 Burlington Gardens, London, W.1, endorsed 'A.D.E.' (February 11).

Lecturer in the Engineering Department (Graduate well qualified by industrial and teaching experience to take Electrical Engineering to an advanced standard)—The Registrar, North Gloucestershire Technical College, Cheltenham (February 11).

Principal of the Crumlin Mining and Technical College—The Director of Education, County Hall, Newport, Mon. (February 11).

Senior Lecturer in the Department of Electrical Engineering in the University of Cape Town—The Ministry of Labour and National Service, Appointments Department, A.9, Technical and Scientific Register, Room 670, York House, Kingsway, London, W.C.2, quoting D.1683.A (February 12).

Expert in the manufacture of Activated Charcoal by the Government of Ceylon—The Ministry of Labour and National Service, Appointments Department, Technical and Scientific Register, Room 670, York House, Kingsway, London, W.C.2, quoting F.5677.A (February 12).

Head of the Pharmacology Department—The Chairman, British Schering Research Laboratories, Ltd., Alderley Edge, Cheshire (February 15).

Joint Assistant Director—The Secretary, Gas Research Board, 1 Grosvenor Place, London, S.W.1 (February 16).

Professor of Mechanical Engineering at the Military College of Science—The Secretary, Civil Service Commission, Burlington Gardens, London, W.1, endorsed 'M.C.S.' (February 18).

Engineer and Surveyor—The Clerk to the Neston Urban District Council, Council Offices, Town Hall, Neston, Wirral (February 28).

Lecturer in Industrial Health—The Secretary, University Court, Glasgow (February 28).

Assistant Engineer, a Civil Engineering Assistant, and a Mechanical Engineering Assistant—The Secretary, Irish Lights Office, 28 d'Olier Street, Dublin (March 1).

Principal, a Professor of Pathology, and a Professor of Physiology—The Secretary, Royal (Dick) Veterinary College, Summerhall, Edinburgh 9 (March 15).

Professor of Physics—The Principal, Heriot-Watt College, Edinburgh (April 22).

Professor in Glass Technology—The Pro. Vice-Chancellor, Benares Hindu University, Benares, India (April 30).

Assistant Superintendent at Regent's Park Gardens, and a Pathologist and Parasitol gist—The Secretary, Zoological Society of London, Regent's Park, London, N.W.8 (May 1).

Chemist for the Intelligence Section of the Plant and Animal Products Department—The Establishment Officer, Imperial Institute, South Kensington, London, S.W.7.

Chemist (Ref. ER/E.216) with industrial experience in the examination and testing of liquid fuels and lubricants, a Chemist or Physicist (Ref. ER/E.217) with a bias towards engineering and with industrial experience, and an Assistant Chemist (Ref. ER/E.218) for general analytical work—The Staff Officer, London Transport, 55 Broadway, London, S.W.1, quoting the appropriate Ref. No.

Lecturer in Biology (temporary) in the Cardiff Technical College—The Director of Education, City Hall, Cardiff.

Teacher of Electrical Engineering Subjects to Higher National Certificate standard—The Headmaster, Dolcoath Technical School, Camborne, Cornwall.

Lecturer in Mining—The Clerk to the Governors, Technical College, Infirmary Road, Chesterfield.

Assistant Lecturer in Physics, and a Lecturer in Mining, at the Huddersfield Technical College—The Director of Education, Education Offices, Peel House, Huddersfield.

REPORTS and other PUBLICATIONS

(not included in the monthly Books Supplement)

Great Britain and Ireland

West India Royal Commission. Report on Agriculture, Fisheries, Forestry and Veterinary Matters. By F. L. Engledow. (Cmd. 6608.) Pp. viii+235. (London: H.M. Stationery Office, 1945.) 3s. 6d. net. [1110

West India Royal Commission, 1938–39. Statement of Action taken on the Recommendations. (Cmd. 6656.) Pp. 108. (London: H.M. Stationery Office, 1945.) 2s. net. [1110

Colonial Office. Development and Welfare in the West Indies, 1943–44. Report by Sir Frank Stockdale. (Colonial No. 189.) Pp. iv+115. (London: H.M. Stationery Office, 1945.) 2s. net. [1110

Ministry of Agriculture and Fisheries. Bulletin No. 131: Winter (Broccoli) and Summer Cauliflower. Pp. 16+8 plates. (London: H.M. Stationery Office, 1945.) 9d. net. [1810